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HUMAN FACTORS IN WORK, MACHINE CONTROL AND EQUIPMENT DESIGN

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EDITORIAL NOTE

The four papers which follow have all been contributed by authors in industry. The first is by the chief engineer of an American aircraft firm, outlining what are, from his point of view, the main essentials of work in the human engineering field if it is to be successful. The second paper is by a specialist in the field of human engineering from another firm, describing the range of activities that he and his colleagues undertake. The third paper is by the head of the Ergonomics Department of a trade research organization in Britain which makes studies and carries out consulting work for a group of factories under the joint sponsorship of these factories and the Department of Scientific and Industrial Research. The article describes the work of the department and the way it has grown since it was first started. The fourth paper is divided into two parts. The first part is by the managing director of a British aircraft firm, who is himself a recognized authority on the theory of manual controls, and sets out some of the important problems of this field in a formal and theoretical way. The second part of the paper describes an experiment carried out by two research workers attached to the firm under a research contract with the Ministry of Supply.

It will be evident from these papers that the task of designing work and equipment in such a way that they are best fitted to the capacities of the operatives concerned is a matter which demands specialized knowledge and experience and that very substantial advantages can be gained in many cases if persons possessing such knowledge are employed either directly or as consultants.

HUMAN FACTORS ENGINEERING*

An Aircraft Company Chief Engineer's Viewpoint

By Carlos C. Wood

Chief Engineer, Long Beach Division, Douglas Aircraft Company, Inc.

Essential features of successful work on human factors in machine and equipment design are summarized and discussed. Stress is laid on the fact that objective, quantitative knowledge of these factors must not only be obtained by research but must also be applied; that it must be incorporated during the design stage of equipment development; and that the worth of such knowledge and the incorporation of it into equipment design must be assessed in terms of effectiveness and cost. This type of knowledge has application not only to the operation and use of equipment, but also to support activities such as maintenance, training and repair.

§ 1. Historical Antecedents

There is so much discussion these days of human factors that we may tend to forget that this field is older than recorded history.

When man first started augmenting his own muscular powers by the use of tools, human factors entered in the man–tool complex. The size, shape, materials and techniques of the axe, its handle and its use, have developed over thousands of years. The weight distribution, the curve of the handle to minimize strain, the shape of the handle to prevent slipping, cross section to provide solid grip and prevent twisting, all represent human factors engineering of the highest order. Thought will show that all successful tools have been subjected to the same type of development.

The past few hundred years have seen progressively greater augmentation of human capabilities by the use of machines which exploit various power sources. These have brought ever greater problems in the man–machine complex.

The last few decades have seen great acceleration in the complexity, speed and capacity of machines to the point that we now appear to be crowding human limits of understanding the complexities, reacting to the exigencies brought about by the speed, and withstanding the physical stresses, such as acceleration, brought about by the capacity of the machines. This has put us in the position of having to ' engineer in ' human factors solutions *prior* to construction, instead of waiting for the previous slow trial and error solutions.

§ 2. Scope and Definition

There are some who question the necessity for continued growth in the human factors engineering field, as the new missile age will just require a properly shaped push button. This I do not believe, because human factors are vitally involved in the success of all of the various phases of any endeavour involving the man–machine relationship including for any machine, firstly,

* Condensation of an invited paper presented at the Fourth Annual Human Engineering Conference sponsored by the U.S. Navy Office of Naval Research.

production; secondly, maintenance and adjustment; thirdly, repair; and fourthly, operation and use. Experience in the missiles field is rapidly pointing out the need for great additional sophistication in the human factors field in the production, maintenance and adjustment, and repair areas when the human operator is removed from the machine. Work in these areas must be *completely correct* for a missile, because we lack the analytical and discretionary ability of the human operator to keep an *almost* correct machine operating.

Considerations such as these have led us in the past few years to modify our concept of the areas to be covered by human factors engineering. In the past much attention has been paid to the problems of the operator, such as the flight crew hurtling through near space at many times the speed of sound. This is great, glamorous and necessary. But, in the light of today's and tomorrow's man-machine (or weapon) systems, it is no longer sufficient. Simply stated, our present view is that: *Human factors engineering must concern itself with all operations in which personnel are involved from the time of initial start of one mission until the time of initial start of the next mission.* This obviously is a task of much wider scope than is often considered.

So far reference has been to 'human factors engineering', and the reader may be wondering if this is a misnomer for much human factors work which might better be called research. Research is certainly essential. It is the required foundation for engineering in any field. But research is not sufficient. The job of an engineering department is to produce design information required for the production of material articles. Philosophically speaking, these material articles to be successful must be useful enough to return to society more than they cost society. Crassly speaking, these articles must return a profit to the commercial organization if the latter is to succeed. Thus my end interest must be in the engineering aspect of the human factors endeavour.

I would define human factors engineering as the application of objective, *quantitative* information on human behaviour to the design of systems and components in order to achieve maximum effectiveness at minimum cost in the operation and support of those systems in the field.

§ 3. Six Essential Features

The preceding definition includes what I consider the six essential features of effective human factors engineering. None of these can be slighted if successful systems are to be designed. I shall discuss these six essential features in turn.

3.1. *The Application, as Opposed to the Mere Obtaining, of Information*

Information must be applied to design problems. This point gets into the basic question of the primary responsibilities of the person we call a human factors engineer. I know that there is considerable disagreement on this point at all levels and even among the people who call themselves human factors engineers, and that I am opening the door for some serious objection. From my standpoint, however, as a part of engineering management, the human factors engineer has the same responsibility as any other

engineer. An engineering department's primary job is to design and/or develop some system, component thereof or other equipment. The engineer's job is to apply information in his particular area of specialization to the solution of some practical problem involved in the design of that equipment. The human factors engineer has the job of applying information about human behaviour to the solution of the design problem in question. I do not mean to imply by this that he must actually go out and build the equipment. Whether he builds it or whether he gives the answers to someone else who builds it is not important. The critical item here is that the information must be applicable to the specific problem at hand. It is not sufficient to provide general information about human behaviour that must then be interpreted by someone else as to its implications for the specific situation at hand. In addition the answer must not only be specific but it must also be consistent with engineering feasibility. This does not necessarily mean that he must be a hardware engineer as well as a human factors engineer but he must work with the hardware engineer enough to insure the practicality of his recommendation.

We in engineering management want, of course, to be sure that the answers we get are right. We also realize that there is a shortage of information in this field and hence that there is a requirement for the work of what one might call the human factors scientist in conducting research to obtain the necessary information. Both responsibilities may be assumed by the same person although how far this happens will vary with the requirements of the particular organization. But the question of how much research should be supported is always a question of the most efficient and economical way to obtain the needed information. In certain cases it is best to conduct the research ourselves. Sometimes it is more economical to buy it. There are even times when, because of the relative unimportance of the solution in comparison to the cost of obtaining it through research, we may consider it best just to accept the best guess. I realize, of course, that there are dangers in this, particularly in the fact that we may not recognize the importance of certain problems. It is important, however, for human factors engineers to realize that even in designing the most efficient system for the customer we must consider the cost of obtaining the information necessary to design it ; that we cannot afford, from the customer's or our own standpoint, to support any and all research one might want to do.

3.2. *The Requirement for Objective, Quantitative Information*

The question of research leads me directly to the second major point —the requirement for quantitative information. I want to clarify what I mean when I say we need quantitative information for engineering purposes. When this statement is made, everyone agrees, but I find often that they are not thinking about the same thing. The type of information that is needed for engineering purposes is what one might best describe as mathematical formulations of functional relationships between different variables. The important factor here is this functional relationship, on the basis of which predictions can be made. Very precise measurements of isolated points tell us little or nothing about functional relationships. We must have tools by means of which we

can predict, with some degree of accuracy, what will happen in a situation that we have not been able to observe in the past.

Most human factor specialists, having been trained not as engineers but as scientists, think more in terms of the exact correctness of their data than in terms of their applicability. In fact it has been said that to be concerned about applicability of data is to be non-scientific. Consequently, the literature in the field of human factors engineering is full of information that is apparently very correct but which represents only one point on what should have been, to be useful, a curve showing how one variable is functionally related to another. However, seldom if ever are we faced with a design problem with conditions that exactly duplicate the laboratory situation and hence those data do not help much in design. We are desperately in need of what one might call mathematical models for describing human behaviour as it relates to equipment design problems. Of course, the more refined these are the better, but the refinement can follow as we go along. This is not different from what happens in the rest of the field of engineering. Much of the data we use are trend data. We often wish they were better, but even so they are better than none at all. In my opinion human factors engineering is being held back more by this deficiency than by any other. It would appear that there is more to be gained at the present by attempting to put the data that are now available into the form of such meaningful functional relationships than from collecting new scattered points. If this were done we would be able to use the data at the time we need it most—before the system is designed.

3.3. *Incorporation of Information During the Design Stage*

The major effort in human factors engineering must be accomplished while the system is undergoing its initial planning and design phase. In one sense I am sure this needs little emphasis ; everyone is aware of this. At the same time, though, I do not think that anyone would say that this is being accomplished adequately today. We are emphasizing this practice at Douglas and I know that other companies are doing the same, but I know that this is not being routinely accomplished in system and system component development. It is a must, however, not only because we cannot afford costly retrofits but also because from a time standpoint it is often impossible to make basic changes after the system has been designed. Some of the primary decisions concerning manual versus automatic functions and manned versus unmanned systems can be made and incorporated in the system only in the early design stage.

I want to point out here, however, certain limitations of which one must be aware. It is never possible to answer all the detailed questions at the beginning of the design process. The design of a system is a continuous process of refinement of approximations—from the gross to the detailed. You cannot afford to wait until you are 100 per cent certain that your decisions are correct before you start the design process. I know it is difficult to make predictions about human capabilities in the operation of some system when only very gross ideas or estimates of the system are available. But someone must do so. The human factors engineer should be in the best position to do this. I might even add that we expect some mistakes. In other areas of engineering we often have to settle for the best guess and sometimes *this* is

incorrect. The same will be required in human factors engineering. We want the best information we can get but we must have answers. However, we expect those answers to be only as precise as the stage of engineering design and development warrants.

3.4. *Use of System Effectiveness and Cost as the Criteria for Design Decisions*

Engineers who specialize in specific areas, such as propulsion, controls, electronics and the like sometimes have difficulty accepting decisions that seem to be contrary to their recommendations. I am sure that human factors engineers are no different. It is difficult for all of us to see beyond our own areas of special concern. Sometimes we forget that it is the total system that is important. Design decisions, however, must be based upon the over-all effectiveness and cost of the system. Increased performance in one subsystem might cause decreased performance somewhere else. In other words, human factors recommendations, like others, must be considered and should be stated in terms of the potential effects upon total system effectiveness and not just in terms of human performance, comfort, or safety. The fact, for example, that a new instrument could be read in a half second as opposed to three seconds for an old one would not justify a change unless it could be shown that a requirement existed for that instrument to be read in less than three seconds. In short, the efficiency of individual components including the human component is important only in so far as it effects over-all system efficiency. The same is true of such things as comfort and safety.

However, one must be very careful to take into account all the factors which influence over-all system effectiveness. One cannot consider a single mission alone. The effects of such things as comfort and safety on morale, as this affects the efficiency of later missions, even re-enlistments, etc., must be taken into account. It is also important here to distinguish between the effects and importance of different factors in different types of operation. In a commercial airplane, for example, comfort and safety must be considered quite differently than in military aircraft. This is not to say that military personnel are any less important than civilian personnel but only that the purposes for which the particular systems are designed are different. In a sense, even in the commercial airliner design decisions concerning safety and comfort must be made on the basis of efficiency. The airlines must pay enough to insure comfort and safety so that people will ride their airplanes. At the same time they must keep fares low enough to be attractive.

3.5. *Consideration of Effectiveness and Cost in Terms of Operational Use in the Field*

Systems must be designed to operate under the conditions that exist in the field. This particular point is one that perhaps needs to be emphasized more to hardware-type engineers than to human factors engineers. We engineers are very much inclined to build systems for operation under ideal rather than actual conditions. We are inclined to ignore the ways human beings behave in field operations, which is perhaps the major source of discrepancy in the two situations. Hence there is an urgent need for human factors specialists to provide information on how people influence system

operation under field conditions and on how to take these factors into account in designing the system. Much of the human factors information currently available is based upon laboratory studies that are not strictly applicable under operational conditions. I am not saying that we do not need this laboratory information or that we should not use it, but I am saying that we need to find ways of showing how these laboratory data are influenced by factors that come into play when the system goes into operational use. We need more follow-up work in evaluating systems in operational use, in an attempt to isolate and specify the effects these operational factors have upon the efficiency of various design features. We have found many times in the past, for example, that an 'ideal' automatic system was far less effective in field use than a less perfect manual system.

3.6. *Human Factors Engineering in System Support Activities*

In the past ten years or so considerable progress has been made in the field of human factors engineering. The major portion of this has been directed towards problems of the so-called 'operator'—the pilot, the bombardier, the navigator, or the controller in ground systems such as air defence or air traffic control. Even in the quite recent past many people have cast rather critical glances at the build-up in human factors engineering groups because they have said we are moving more and more toward unmanned systems and that, therefore, the requirement for this type of effort will soon be eliminated. Fewer people are making those statements today. We are rapidly beginning to realize that the trend toward increased automaticity is by no means decreasing the requirement for human factors engineering. Instead it seems to be greatly increasing this requirement. Missiles engineers, perhaps belatedly but none the less certainly, are becoming well aware of this fact.

We all are becoming aware of the fact that when you take the man out of the actual operation of the system you magnify the support responsibility many fold. Requirements for accuracy are greatly increased. Errors that might have been insignificant in a manned system might well be catastrophic in an unmanned system. The human being readily corrects his errors but only the most sophisticated machine can accomplish this activity and then to only a very rudimentary degree. I am not suggesting here that we have gone too far toward automizing systems. The requirements for greater precision and higher performance leave no other course. I am saying only that with this increase in complexity of systems have come increased demands for human factors engineering in the various support activities. As I see it, the biggest void in human factors engineering today and one which must be met in the near future is that imposed by tne need for greater efficiency and accuracy in the performance of various ground support activities.

Some of these points may not seem glamorous or even important to many people but they are very important to those of us who are charged with management responsibilities in the engineering field. The sooner we can get human factors data on to the drafting tables, slide rules and into the IBM machines the better off we shall be. We are being forced to operate at the limit of human understanding, capabilities, stress and perception in practically

everything we undertake for the future. We must be able to apply continually greater engineering finesse in the human factors field to continue our progress in the over-all fields of engineering and product development. We need more basic information from research, and we must be in a position to apply it rapidly to our evermore complex practical day-to-day problems.

On décrit et discute les caractéristiques essentielles d'un travail couronné de succès sur les facteurs humains dans la construction des machines et des dispositifs. On souligne que 1° la connaissance objective et quantitative de ces facteurs doit non seulement être obtenue des recherches, mais aussi appliquée, 2° qu'elle doit être réalisée pendant l'étape de construction dans le développement du dispositif, et 3° que la valeur d'une telle connaissance et sa réalisation dans la construction du dispositif doivent être estimées du point de vue de l'efficacité et du prix. Ce type de connaissance peut être employé non seulement pour le fonctionnement et l'application du dispositif, mais aussi pour supporter les activités telles que la conservation, instruction et réparations.

Die wesentlichen Momente einer erfolgreichen Arbeit an menschlichen Faktoren in der Maschinen- und Apparaturkonstruktion werden zusammengefasst und erörtert. Es wird hervorgehoben, 1) dass die objektive quantitative Kenntnis dieser Faktoren nicht nur durch Forschung erzielt, aber auch angewandt werden soll, 2) dass diese Kenntnis während des Konstruktionsstadium der Apparaturentwicklung zur Verwirklichung gelangen soll, and 3) dass der Wert einer solchen Kenntnis und deren Verwirklichung in der Apparaturkonstruktion vom Standpunkt der Wirksamkeit und der Kosten geschätzt werden soll. Dieser Kenntnistyp kann nicht nur für den Betrieb und Anwendung der Apparatur verwendet werden, sondern auch zur Unterstützung solcher Tätigkeiten wie Wartung, Schulung und Reparaturen.

INDUSTRIAL APPROACHES TO HUMAN ENGINEERING IN AMERICA*

By J. A. Kraft

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Examples are given of the many different ways in which human engineering programmes are introduced, staffed and developed within industry. The present scope of such programmes in the aircraft industry is outlined, and the future expansion of human engineering studies is discussed.

§ 1. Introduction

As is generally known, there are numerous approaches in industry to human factors research and human engineering. I plan to discuss how some of these programmes originate, where they are located in the organizational structure, what kind of skills they use, what they do, and some of the advantages and disadvantages of selected approaches. I also plan to mention, briefly, a somewhat idealized industrial application of this speciality.

I have selected the aircraft industry as the focal point for most of my remarks since that is the one with which I am most familiar, and since it was one of the first to recognize the need for human engineering.

§ 2. Industrial Origin

Prior to 1950, few industries of any kind had formally established human engineering capabilities, as such, although many were carrying on such work in other programmes. Since that time, a rather sizeable number of companies have established such programmes or hired specialists to perform the required services. In 1954 the growth of human factors groups in aircraft industries became most apparent, and, today, virtually all of the major airframe and supporting companies have programmes employing from one man to as many as thirty-five.

Some of these programmes were started because top engineering management recognized that human engineering was needed to improve product design and operator efficiency, to streamline maintenance of their product, to increase its operational safety, to satisfy customer contract requirements and to increase sales. This seems like an ideal way for a sound programme to get started since it begins with a policy decision to hire qualified human factors specialists and to build up the capabilities of the staff to satisfy recognized needs. I know of several companies who have used this approach successfully.

Other programmes have evolved through the company use of consultants retained for specified jobs, through the use of specialists working in government agencies, or through close contact with university human factors programmes. These companies eventually form their own programme and, in some cases, hire on a full-time basis the consultants or the government or university specialists

* A paper presented to the National Symposium on Human Factors in Systems Engineering; sponsored jointly by the Human Factors Society of America and the Institute of Radio Engineers (Philadelphia Section), at Philadelphia, Pennsylvania, December 3–4, 1957.

who have assisted the company on various projects. This, too, seems like a good way to get started because management first sees the concrete results of the services before the decision is made to establish a company programme.

Some programmes have evolved from within the company as personnel identify themselves with a human factors problem or specialize in certain areas of human engineering. In some cases, qualified specialists have been engaged in other activities such as personnel, medical, industrial engineering, or other staff functions. Such people are called on from time to time to assist in solving a pressing human factors problem. Eventually a programme can grow out of such varied activity but in many of these cases the work is ill-defined and most susceptible to individual whims. The prognosis for success of programmes which have been established in this fashion is not good because they are often taken for granted by management and because they frequently have no well defined status within the company organization.

In certain instances these human factors groups evolving from within come about as the result of a requirement in the prime contract and when this requirement is satisfied the personnel who have conducted the activity return to their former assignments.

Still another way for a programme to develop is through a company undertaking a human factors contract project which is pertinent to other company efforts. A group is then assembled to conduct the necessary research and, if it is successful, it may perpetuate its existence through a continuation of contract or company supported research activity of a specialized nature. There are dangers inherent in such an approach because the programme may tend to become too one-sided and fail to provide essential services to other engineering efforts. Programmes such as this may die out when contract support disappears or when business begins to slacken, unless management has recognized the necessity for such research.

§ 3. Organizational Placement

The manner in which the human factors programme develops may have a great deal to do with its placement in the organizational structure. Several of the groups now in existence in the aircraft industry are established as line organizations or have line responsibility in that they have design sign-off authority. In these cases, equipment or systems designs must be approved by the human engineering group if human considerations enter into any part of the design. In this situation the human engineer can make the most effective contribution if his judgement is called for at the earliest stages of design as well as throughout the design programme.

However, in most cases, human engineering programmes are considered to be staff or advisory functions and exert influence in a more indirect manner. Such groups are called on frequently for consultation on human factors problems when they are recognized by the design engineer or, as a matter of course, in the design effort. Or, such groups might solicit work by educating other engineering organizations to utilize their services. They may also conduct small-scale experimental studies to secure the required information or they may participate in broader research studies not directly of a human engineering nature. The danger, of course, of having these services on an ‘ on call ’ basis is that immediate answers are expected to be given by the ‘ experts ’. Where such answers are

readily available in the literature with which these specialists should be extremely familiar, this is not a bad working situation. But, too frequently, the answers must be ‘ dug out ’ from many sources or established experimentally. Here is one of the real tests of the degree of acceptance and understanding of human engineering. If the persons needing the required information have asked for it sufficiently ahead of time or if they have the patience to wait for a sound answer—then the answers received can be genuinely useful. However, from time to time, the human factors specialist must make educated guesses based on previous experience and training and the most that can be hoped for is that these guesses will be right many more times than they are wrong.

It is difficult to recommend the place in an industrial organization for a human engineering programme since few companies are structured alike and since needs vary widely. In the airframe industry, one finds human engineering specialists or groups in advanced or preliminary design, on electronics and control staffs, in operations research, in staff or systems engineering, in professional services groups, in design safety, in cockpit furnishings, on technical specialties staffs, in servo-electronics, in applied research, on reliability staffs, in equipment design, and in advanced or long range planning. This list is by no means exhaustive, but it re-emphasizes the many applications of this specialty.

Most of the organizational placements locate the services at a level sufficiently high enough to be effective and, in many instances, provide straight line reporting to the Chief Engineer or a high level engineering executive.

§ 4. Skills Used

The present composition of the various groups is also revealing. In years past most people calling themselves ‘ human engineers ’ came from psychology. Today, although a great many psychologists are still employed in this capacity, there are also many electrical, mechanical and aeronautical engineers, as well as anthropologists, physiologists, industrial designers, mathematicians, physicians and pilots. The multi-discipline nature of these groups in industry is becoming even more pronounced, although engineers and psychologists still account for the greatest numbers.

§ 5. Present Activities

A list of the specific activities of industrial human engineering programmes is too lengthy to enumerate. However, a number of examples of basic research, applied research, systems research, and day-to-day activities will give you some idea of the scope.

The classification of basic and applied is somewhat arbitrary but the following studies or projects could be considered basic in the sense that they are designed to establish principles or to provide information applicable to a wide variety of systems or designs.

Some examples of basic studies are the investigations of human tolerance to high thermal and noise stress; human factors problems associated with aircrew stress, fatigue and confinement during extended flight; human tolerance to transverse g; human pilot control dynamics; visibility and perception studies; target recognition; human operator response and operator loads, and the psycho-physiology of space flight.

Studies which might be called applied include research on escape seat design; display analysis; cockpit and missile console design; simulator design; and the development of proficiency measures and design standards.

One major project which embraces the *weapons systems approach* is the Army–Navy Instrument Programme wherein two aircraft companies, under Army–Navy Contract, are coordinating the work of many industries in an effort to establish flexible integrated display systems for airplanes, helicopters, tanks, and missile control consoles.

In addition to these study efforts, most groups have *day-to-day activities* of a service nature. These include such work as cockpit evaluation, design review, task equipment analysis, preparation of training material, preparation of human factors data for engineering design application, consultation with designers on human factors problems, circulation of human engineering documents, conducting literature search in specified problem areas, and the like.

§ 6. An Idealized Approach

Ideally speaking an industrial human factors programme should be able to conduct some essential experimentation. It should also have the closest possible working relationship with advanced and preliminary designers and it should be able to carry on day-to-day service activities for all of engineering.

Direct work lines should also be established with the company's flight safety programme, the various staff engineering functions, equipment and furnishings designers, ground handling equipment designers, field service, engineering flight test, electronics systems staffs, technical services groups, technical handbooks, operations research, sales engineering, and the various project groups.

Sign-off responsibility on basic designs is highly desirable, particularly, if the human factors people have participated in the developmental aspects of the system. Short of that, the next best contribution can be made through assignment of human engineering coordinating responsibility to a qualified group or person who, in turn, becomes an active participant in all phases of system design, equipment design, operation and utilization wherein the human role must be considered.

In order to be effective in industry the information supplied and the recommendations made by human factors engineers:

(*a*) must be appropriate to the problem at hand;

(*b*) should consider the entire systems requirement;

(*c*) must be presented in an understandable manner to the user;

(*d*) should be quantitative, when possible;

(*e*) should be consistent with the overall design requirements; and

(*f*) should be feasible for subsequent production, sales, operation, and maintenance.

The industrial human engineer may wear many hats—he may be a scientist in one situation, an expertizer in another, a practical engineer in a third, an information specialist in a fourth, and a salesman in still another.

§ 7. Future Prospects

Regardless of the hat he may wear, there now is every reason to believe that the human factors research specialist will find his proper place in industry.

The events of the past several months have brought about another 'new look' in our defence programme which may come to be regarded as the 'scientific look'. Basic research, as such, is no longer to be avoided or camouflaged. In fact, such research is once again gaining the respect and support which is so vitally needed before any other type of research can be fully effective.

Basic research is as essential to human engineering as it is to any other engineering or scientific activity. With the right attitude toward such research and the proper support from government and industry, it is conceivable that the next ten years may see a phenomenal growth in human factors research programmes.

With the prospect of space flight leaving the realm of science fiction and approaching reality within our generation, more information must be gathered about the human organism than was ever required for other modes of travel. This is a universally recognized fact. It is one that does not have to be sold and resold; it need not be sugar-coated. Even the most product-oriented or technically minded person may now recognize that if man is bound to this planet in years to come, it will be because of the lack of full knowledge of man's capabilities and limitations, and not because of ignorance of the engineering principles necessary to project him into space. Basic human factors research can provide much of this knowledge.

In addition to this requirement for more basic human behavioural information, it will also be a requirement that space travel equipment be built around the human—more so than has ever been the case in the past.

We will need to know the maximum g loads which can be tolerated, for how long, and the cumulative effects of such loads. We must know a great deal more about how men will perform in a weightless environment, under periods of long term confinement, in extremely stressful circumstances, with many physical and psychological deprivations. We must know more about the characteristics of crews who can perform satellite station duty or space travel. We need to know more about the kind of and the amount of tasks which can be performed for long periods. We must be able to provide required personal equipment for survival during routine flight, in emergencies, and upon arrival at destination. We also must develop more group behavioural information.

These are but a few of the areas which must be explored. Yet we have just barely scratched the surface in coping with today's human factors problems in air travel, and now we must be prepared to consider a whole new realm of problems in space travel.

The government cannot carry the burden alone in supporting and conducting the required research. Business, industry and universities will also have to make their contributions. For these reasons, I believe that no matter what we call this endeavour, we can expect human factors research activity to increase in government and in profit and non-profit organizations or institutions.

One further point should be mentioned. It is not unusual for proponents of human engineering in industry to spend a great deal of time, initially in selling their programme and subsequently in keeping management convinced of its value. I believe this can be overdone and I wholeheartedly support the

statement made recently by an executive of a large company, that less time should be devoted to defending human engineering and more should be spent in producing results which are understandable and so obviously worthwhile to engineers that they speak for themselves.

Des exemples sont donnés des façons différentes par lesquelles les programmes de mécanisation humaine sont introduits, pourvus de personnel et développés dans l'industrie. L'étendu de tels programmes dans l'industrie aérienne est tracé et l'expansion future des études de la mécanisation humaine y est discuté.

In dem Artikel sind Beispiele von vielen verschiedenen Methoden der Aufstellung von Programmen der 'menschlichen Technik' (human engineering) in der Industrie, sowie über die Auswahl des Personals und die Weiterentwicklung der diesbezüglichen Pläne, gegeben. Der gegenwärtige Stand solcher Programme in der Flugzeugindustrie wird beschrieben und die zukünftige Ausdehnung von Untersuchungen über 'menschliche Technik' wird besprochen.

PRODUCTION PROBLEMS IN THE SHOE INDUSTRY

THE WORK OF THE ERGONOMICS DEPARTMENT OF AN INDUSTRIAL RESEARCH ASSOCIATION

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It is suggested that there are many industries including shoe manufacturing where automation is not likely to spread rapidly. The reasons for this are enumerated.

The way in which Ergonomics work has developed in the shoe industry is described with examples of the redesign of sewing machine controls, the selection of operatives and the study of production control systems.

§ 1. INTRODUCTION

THERE appears to be a widespread belief that in the near future production problems in factories will be problems of automation, where the typical worker will either watch dials and turn knobs or trace faults in elaborate mechanical and electronic systems. In the shoe industry, however, as in many other industries the basic production unit is likely to remain the skilled operative actually using a machine to do the work rather than watching a machine working. The transition from one phase to the other only occurs when the industry concerned has the following characteristics:

(i) Raw materials of known consistent properties.

(ii) A product for which quantitative specifications can be drawn up with some precision.

(iii) Long runs of a standard product.

(iv) Capacity for high investment in capital goods.

None of these conditions obtain in the shoe industry. Much of the raw material is natural (and therefore variable), there are no specifications for shoes in the sense that these are provided for all engineering products and the industry is essentially a fashion trade with all the rapid changes in product which this implies. Few firms are likely to have sufficient financial status to indulge widely in the purchase of complex machinery since the average size is less than two hundred workers and there are only nine out of a total of a thousand firms which have a staff of more than one thousand people. The shoe industry provides an interesting contrast with the mineral oil refining industry which in 1951 was about the same size in terms of value of gross output (Census of Production 1951). The fixed investment per person employed in the former was about £15 and in the latter nearly £3000. These are extreme examples, of course; the mineral oil refining industry has expanded more rapidly than any other since the last war and the figure for the shoe industry is artificially low because of the practice of hiring machines. The point I wish to make is that although mineral oil refining is an example of the type of industry where automation is spreading rapidly, shoe manufacturing is typical of another large

section of industry where this is not the case. Only 13 per cent of the industrial establishments in this country employ more than 200 people. More than half of the industrial population work in establishments with less than 500 employees.

Although the smaller industrial firms are not faced by any problems of automation, production methods are changing rapidly and bringing their own problems. Shoe manufacturing, for example, is reversing the popularly accepted trend of mass production industry. The variety of product has increased considerably since the war. This and the labour shortage are the modern trends with which we have had to deal in the past few years.

The task of the Ergonomics department of the British Boot, Shoe and Allied Trades Research Association has been defined as the study of " anything to do with productivity in the shoe industry ". The main variables we have identified in this field are shown in Fig. 1. The practical convenience of the wide terms of reference can be illustrated by a brief history of work done by the department on sewing machines.

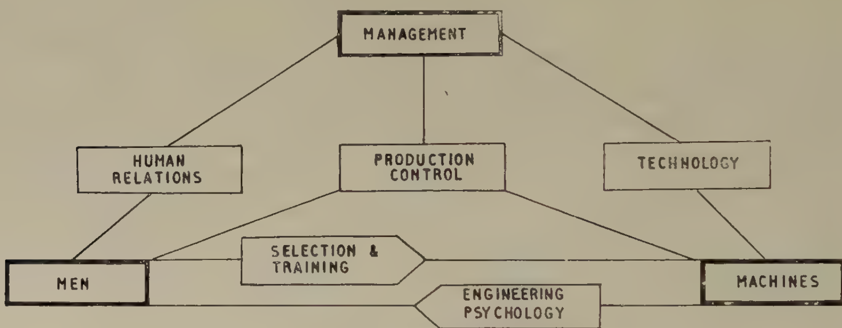

Figure 1. Productivity variables.

This began with an investigation of the machine controls in relation to the operator, which was clearly a case study of what is known in America as 'engineering-psychology'. It was almost immediately obvious that a psychologist alone cannot interfere with machinery used in factories. He needs the cooperation of a technologist who, on the basis of long experience of the industry, can advise on the practical feasibility of changes which are suggested on theoretical grounds. The usefulness of any changes which are made must be validated on the factory floor, but experimental situations can only be introduced into factories where there is a reasonable level of mutual respect and cooperation between operatives and management; in other words, in factories where the 'human relations' situation is satisfactory. In taking measures of performances during investigations in different factories it became clear that fluctuations in supply of work can have a greater effect on rate of production than machine control modifications and for this reason production control became another topic to be studied. It was also found that full consideration for the operator often means adding considerably to the complexity and therefore the cost of the machine. Except in cases where health hazards are demonstrable, practical compromise between adapting the machine design or adapting the operator by training must always be made on economic grounds. Thus, a research unit which began four years ago with the study of machine

controls has almost of necessity become an interdisciplinary department including among its staff specialists in engineering psychology, training, shoe technology, production control, work study and engineering.

The work on sewing machines can be used to provide examples of practical procedures in these various fields.

§ 2. Engineering Psychology

Three changes in the controls have been investigated.

2.1. *Operatives' Posture*

For sewing machines used in factories the position of the treadle in relation to the sewing machine is as shown in Fig. 2. In order to get both feet on the treadle and the hands near the needle the operative must either bend forwards at the waist or bend the legs backwards at the knee in addition to sitting with the trunk slightly twisted. Most operatives adopt the posture shown in Fig. 2

Figure 2. Posture of sewing-machinist at a standard bench.

where the left foot rests on the safety bars and the right foot only is used to operate the treadle. This has been standard practice for many years and in most cases it was not until the machine controls were made a specific topic for study that anyone realized that there was anything wrong in this situation.

Using anthropometric data an optimum position for the treadle was calculated and recommended to shoe manufacturers. The adjusted treadle would be approximately under the left foot in the position illustrated in Fig. 2, that is, more to the left, farther under the machine and about four inches higher than the standard position, so that operatives can sit with the thighs horizontal and reduce pressure on the soft tissues of the legs from the front of the seat (Åkerblom 1954).

This problem has not yet been finally solved since there are two conflicting factors in determining the distance of the eyes from the needle. It is desirable to have the hands fairly low so that the forearms are not inclined upwards, but it is also desirable to have the work fairly near to the eyes to ease the fine visual discrimination. Experimental work is proceeding to determine the optimum solution.

2.2. *Needle Position Controls*

The industrial sewing machine operator can only control the position in which the needle stops by taking one hand away from the work and turning the balance wheel. It was found that the mechanical engineering problems of producing a power transmitter to control needle-position had been solved. The designers, however, had not solved the human engineering problem of producing adequate controls for this transmitter. These originally consisted of a double-foot treadle which rotated on two perpendicular axes and which required anatomically inefficient leg movements and psychologically inefficient control processes. A new treadle system was designed and validated after a study of the operational requirements of the task. This investigation has been reported in greater detail in another paper (Singleton and Simister 1957).

2.3. *Variable-Speed Controls*

The various types of sewing machines used by the shoe and clothing industries are normally driven by an electric motor through a clutch and brake mechanism. When the treadle is pressed the clutch moves on to a flywheel attached to the electric motor running at a constant speed and the machine sews; when the treadle is released the clutch plate moves off the flywheel on to a fixed brake and the machine stops. This is an excellent simple mechanism for the production of rapid acceleration and braking—an important feature since in the shoe industry the machines will normally start and stop about one thousand times per hour during routine production work. However, the mechanism has one serious defect from the human engineering point of view. It is obvious when looking at any style of shoe that very few of the rows of stitching are straight lines. The operator must sew along lines of differing curvature and to do so rapidly and accurately she must vary the machine speed. Using the standard transmitter the only way to sew at speeds other than maximum is to slip the clutch. This is equivalent to driving a car by keeping the accelerator hard down and adjusting speed by slightly depressing the clutch pedal. This is possible, but it would not be easy and would probably be very tiring. Bearing in mind the frequency of stops, starts and speed changes the stitching of shoe uppers must also require considerable effort and concentration from the operator over her nine-hour day.

Simple mechanical tests demonstrated that the treadle moves about one twentieth of an inch from zero to full machine speed and that the pressure required for a given speed within this range varies considerably. The machine is, in fact, equipped with a complex pressure and amplitude control with variable characteristics.

Psychologically the solution is simple; to design a control such that to attain any particular speed the treadle must be moved through a perceptible distance with a consistent change of pressure. This has proved to be a very difficult engineering project, but this is not relevant except that it illustrates the point already made concerning the cost of good machine controls. Mechanical or Design Engineers are regularly criticized for their failure to consider the machine operative, but it must be remembered that the cost of the product is one of the most important factors which determines the design of a machine. The task of detecting mistakes in control design and recommending appropriate changes is only the first (and incidentally much the easiest) part of the job of the industrial engineering psychologist; he must then proceed to produce data concerning the effect of the changes he recommends. This data can be used to validate the changes, to convince the engineers or managers that the changes are worthy of consideration and to provide a cost target for the new controls. Appropriate data are difficult to obtain since improvements in machine controls can have at least four separate effects:

(1) improvement of productivity and/or reduction of fatigue,

(2) improvement of quality of work,

(3) reduction of accident rate, and

(4) reduction of training time for the operative.

The relative importance of these factors depends on the particular case. To obtain data on each of them is itself an expensive undertaking which can rarely be justified unless the machine concerned is used very widely in one or more industries.

Considering industrial sewing machines, there are about 100 000 in use in this country so that large-scale scientific investigations are justifiable. Variable speed controls have been investigated by comparing them with standard controls in laboratory experiments using synthetic tasks and by substituting them for standard controls in factory trials on production work. The results of these investigations will be published in a later paper.

§ 3. Selection and Training

In the selection of sewing machine operatives two separate attempts are being made at the present time. The first is to produce a battery of tests which will predict a girl's final skill as a sewing machine operator. This is an ambitious task which has been pursued intermittently for some years without very much success.

The second is to detect those girls who will not stay in this occupation either for intellectual or emotional reasons or because they tend to be unusually clumsy when carrying out manipulative tasks. Most of this group drop out during training-courses and never reach production work. Nevertheless it is important to prevent them from starting on the courses since failures are bad

for the morale of others and tend to discourage those thinking of offering themselves for training. This work is also at the experimental stage but it is likely to be successful more rapidly and more completely than the first project.

The training scheme for operatives which is sponsored by the Department utilizes the progressive part method developed by Seymour (1954) and will be described in a later paper in this Journal.

§ 4. Production Control

The way in which applied work in this field tends to extend to a variety of topics has already been mentioned in relation to the skills necessary for the smooth running of an ergonomics department. The same point can be illustrated by considering what happens within the factory, starting again from machine-control investigations and the measurement of productivity. There is no doubt that the efficient supply of work to the operative is one of the main determinants of rate of production. One of the working assumptions in every ergonomics investigation is that, when a man is not doing his job efficiently, it is the conditions rather than the man which need improvement. Thus, in an engineering psychology project the primary aim is to improve the machine controls. Similarly, if the supply of work to an operative is not adequate it follows that the foreman is not working effectively and therefore methods of improving his working environment must be considered. This involves the study of machinery lay-out, work transport systems and the planning of work into the department. Studies of procedures in planning departments usually demonstrate that, although there is considerable scope for improvement in organization and methods, part of the trouble can be traced to the sales policy which is rarely determined with sufficient regard for production problems. Thus the ergonomics department became involved in the study of the contrasting demands of sales and production. This may appear to be a long way from the study of machine controls or from what is normally understood as Ergonomics and Human Engineering. However, the problem at all levels is the same in that it involves ensuring that a man is provided with the information without which he cannot do his job efficiently. The operative needs a machine which is designed to provide information concerning what he is doing or will have to do in the immediate future. Thus a variable speed transmitter for a sewing machine makes it possible for the operator to control the speed using kinaesthetic information. A foreman can carry out his duties more effectively if the machinery is placed in a rational sequence and if the work transport system is such that he can easily locate all the material in his department. A production manager or planning officer cannot organize the daily supply of work unless he knows the capacity of the factory for the key or bottle-neck operations and the work content for these operations of all work available for input. A sales manager is not helping his company if he indiscriminately accepts all orders including those which are so small that production costs will increase beyond the profit margin. Once again he needs information concerning the way in which costs increase when the size of lots decreases.

These statements may appear to be trite and obvious, they are certainly not scientific discoveries, they follow logically once the principle that a man cannot take appropriate action unless he is given adequate information, is recognized and accepted. This principle is not followed as closely as it could

be partly because the main art of the industrial executive is to make the right decisions without full information. Nevertheless, it must be true that the probable accuracy of decisions increases in proportion to the extent to which relevant information is considered and it is for this reason that the techniques of Ergonomics, Operational Research, and Scientific Management have been developed and have an important place in advanced industrial societies.

The projects described in this paper were sponsored by the D.S.I.R. and M.R.C. Joint Committee on Individual Efficiency in Industry and were assisted by a grant from I.C.A. Conditional Aid Funds.

Il est suggéré que dans beaucoup d'industries, y compris la manufacture de chaussures, le travail automate ne se répandra pas rapidement. Les raisons pour ceci sont énumérés.

La façon dont le travail ' Ergonomique ' s'est développé dans l'industrie des chaussures est décrite par les exemples de re-modelage des contrôles de machines à coudre, la sélection des ouvrières et l'étude du système de contrôle de production.

Es wird die Behauptung aufgestellt, dass es viele Industriezweige, die Schuhmanufaktur inbegriffen, gibt, wo eine schnelle Verbreitung des Automation nicht zu erwarten ist. Die Ursachen hierfür werden angegeben.

Die Richtung, die die Bemühungen zur Leistungssteigerung in der Schuhindustrie eingeschlagen haben, wird an der Hand von Beispielen, wie Neuentwürf der Nähmaschinen Konstruktion, der Auswahl der Arbeitskräfte und der Untersuchung der Systeme der Produktionskontrolle erläutert.

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THE DESIGN AND INTERPRETATION OF HUMAN CONTROL EXPERIMENTS

PART I by J. D. NORTH
PART II by Z. A. LOMNICKI and S. K. ZAREMBA
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PART I is a general statement of some of the factors which must be taken into account in the design and interpretation of human control experiments. In particular the non-ergodic character of the asymptotic learning state (statistical quasi-equilibrium) demands special precautions and a large amount of data. Attention is drawn to the part played by bias and noise.

PART II is an account of certain experiments with a machine incorporating computer elements so that statistical data may be accumulated rapidly, *inter alia* in the form of mean square deviations. The experiments described relate to the various phases of learning and the regression of mean square deviation on sensitivity in the 'asymptotic' state.

PART I

THE use of tools and machines as mechanical extensions to men has a long history. The optimization of man–machine combinations has continued through this history by an evolutionary process. Modern developments in automatic control have led to the possibility of more and more human functions being taken over by the machine; so much so that the new philosophy of 'automation' almost seems to suggest that everything a man can do a machine could do better. This I suggest is far from the truth. Its acceptance may be due to the fact that the application of modern scientific techniques to the study of the human behaviour in man–machine combinations is of comparatively recent date and the new types of formal models, such as the Mathematical Theory of Communication, the Theory of Stochastic Processes, the Theory of Games and Econometrics in general appropriate to a unifying theory of behaviour, have been the subject of much recent development. The purpose of this paper is to discuss certain aspects of the design and interpretation of experiments with man–machine combinations.

In its most general form we are interested in the behaviour of the model of the type shown in Fig. 1, where behaviour or performance is characterized by the relations between X and Y.

Figure 1.

Formally this is represented as follows:

$$X \equiv \{x_i\} \rightarrow \{y_i\} \equiv Y; \quad \{x_i\} \equiv x_1, x_2 \ldots x_i \ldots x_n.$$

The deviation sequence $Z \equiv \{z_i\}$ where $z_i = x_i - y_i$

$$E(Z) \equiv \{E(z_i)\} \equiv \text{the sequence of expected } z_i.$$

Similarly for $E(X)$ and $E(Y)$.

$$Z-E(Z) \text{ is called 'noise'.}$$

The mean square noise $S_n{}^2 = \frac{1}{n}\sum_{i=1}^{n}[z_i - E(z_i)]^2$.

The noise variance $\sigma_n{}^2 = E(S_n{}^2) = \frac{1}{n}\sum_{i=1}^{n}E(z_i)^2 - \frac{1}{n}\sum_{i=1}^{n}[E(z_i)]^2$.

The first part of the right-hand side is the variance of the deviation; the second part is the squared bias. In any experiment, observations are represented by sequences of finite length. An experiment tends to list the correspondences. It is easy to see, for example, that if in the sequence X the x's could take 10 discrete values, i.e. that there were 10 distinguishable states of the x's, then there could be a 10^{100} such sequences of length 100. It will be obvious, however, that some of these sequences have well defined patterns, and that some patterns are more alike than others; that is to say, the whole set of possible sequences can be assigned, albeit by an arbitrary though consistent method, to certain sub-sets in which the sequences are regarded as having been generated in a few cases by a determinate, and in many more by stochastic processes, leaving a large residue to be regarded as amorphous. In a somewhat similar way the relationship pattern between X and Y may be assigned in such a way that there is a one-to-one correspondence between X and Y, in which case the process is determinate, or a correspondence between X and a sub-set of Y's preferable to a sub-ensemble. In this case replication is necessary, since observations of frequency are needed for the estimation of probabilities. We have, however, no prior assurance that these relationships are stationary in time; in fact, during what is sometimes called the learning period, we know that they are not. In order that anything fruitful shall come out of the exercise we must hope to find some more or less permanent relationships to which individual human behaviour converges. I shall suggest that these convergencies contain several different plateaux.

The most general description of such a permanent relationship is that of a strategy as defined in the 'Theory of Games', but unless the strategy can be expressed in the form of a combination of elementary tactics, very few useful results are to be expected from the experiments. The formal representation of a tactic in the sense in which I have used the word would be an operator in the mathematical sense. If the operator is determinate, it transforms a sequence into a sequence (one-to-one correspondence); if the operator is stochastic, it transforms a sequence into an ensemble of sequences. Even in the latter case, however, we may postulate an 'expectation operator' which transforms X into $E(Y)$; it is immaterial whether the operator is linear or not. The difference between the expected outcome $E(Y)$ and its ensemble has conveniently been called noise; if the operator is linear, the noise is independent of the input and depends solely on the operator. The fact that all mechanical realizations of operators are certainly non-linear and stochastic does not prevent us from using linear determinate operators as models. Where disturbances are not too great and where the coefficient of variation (i.e. the size of the ensemble) is small, there is considerable evidence that linear models with suitable adjustments, which have been so successful in engineering, may be used to describe human tactics; but there is equally strong evidence that in human control, the

coefficient of variation is so large even at the end of the learning period that the stochastic operators are required for any useful formal model.

Both on account of the stochastic nature of human behaviour and the complexity of the convergence with experience, a very large amount of experimental data is necessary to extract even simple behaviour patterns. It is necessary to choose some statistic which could be obtained very quickly. Such a statistic is the time average of the squared deviation of a sample sequence, i.e.

$$\frac{1}{n} \cdot \sum_{i=1}^{n} z_i^2.$$

This, as has already been pointed out, is the sum of the square of the bias in the sample plus the average squared amplitude of the noise measured about the bias. Since the bias sequence is an expectation, it cannot, of course, be measured or recorded, but in suitable cases the time average of the mean deviation (*not* the mean modulus deviation) throws some light on the bias. For example, if a velocity control is used with a velocity target, the expected bias is zero everywhere. There does appear to be some evidence that, in a compensatory task, a subject will tend to adopt a strategy as if he were trying to minimize the mean squared deviation, although it is easy to find particular applications where this would not lead to the choice of the best strategy. It is, however, true, in linear systems (and probably for useful non-linear systems as well), that bias has to be traded for noise, since the filter characteristics which diminish one magnify the other in the useful tactical range. Now it is clear that there are two principal ways in which improved performance (and I shall assume that decrease of mean squared deviations means improved performance) can be arrived at:

(1) By reduction in the basic noise, i.e. that the subject tends to form more accurate quantitative judgment of the display, and that having decided what is the appropriate corresponding lever movement, he makes this movement more accurately.

(2) He employs a better strategy, i.e. one which, given the basic noise level and the specified input, tends to minimize the mean squared deviation.

Now the experiments of Hick (1952), Crossman (1953) and Quastler (1955) all suggest, at least in the earlier stages of experience, that the basic noise level depends on the information rate demanded by the strategy, or rather by each and every one of the tactics of which the strategy is composed. But those very tactics which tend to diminish the amplification of the basic noise in many cases demand an increased information rate, and this information rate may be represented by more than one logon or factor; for some of these factors the 'channel capacity' is lower than for others. This particularly applies to predictive factors and is most noticeable where the task involves appreciation and consequent actions different from those required by previous *general* experience. During this stage of learning the subject appears to take this into account. The experiments of Crossman (1956) and Quastler show that this is a passing phase; it is as if a selective information channel were replaced by resonant channels so that the noise tends to be much less, if at all, dependent on the information rate, provided this does not demand an increase of the number of resonant channels. Once this has been achieved, tactics previously unacceptable are immediately to be preferred and appear to be adopted.

Proceeding in parallel with the above learning process is one at an altogether different level, namely the level of meta-information. The term 'meta-information' was introduced by the author for a concept which is described in detail elsewhere (North 1956). Roughly speaking, meta-information is information about a stochastic process itself, as distinct from the information conveyed by observation of the process in action. For example, it is well known that a die may be so loaded that the probability of falling on the several faces is far from equal. Now, physical measurements might be expected to furnish some information about the trivariate probability distribution relevant to defining the position of the centre of gravity. This information would be meta-information as distinct from that to be obtained from a series of sequences resulting from experimental throws of the die. Incidentally, on the supposition that an exact knowledge of the position of the centre of gravity would wholly define the probability of any number being thrown, the meta-information above described is necessary and sufficient for a Bayes solution.

The optimum tactic for minimizing the mean squared deviation, given the basic noise, depends on the input and the mechanical transformation characteristics of the machine. However, insofar as the latter is permanent, learning is a matter of familiarization similar to that which occurs when, for example, a different tennis racket is used, but not all such transformations are permanent in this sense. Provided the changes are parametric and not structural, Hick's experiments suggest that the subject can readily accommodate himself to them, sometimes in a period not much longer than the reaction time as determined from detached experiments.

Something of a much more subtle character is necessary, however, if the subject is to adapt his tactics to the input. In a pursuit task, where the input is independently displayed, the 'recognition' of an input as a member of a class after the presentation of some elements of the input sequence can only be achieved if there are a moderate number of fairly widely separated classes of inputs; in other words, he is drawing upon meta-information about the universe of inputs which he has previously acquired. There is, of course, nothing abnormal in this, since it is common to all human, and indeed much animal, behaviour.

There are many other sources of meta-information; for example, the distinction between a bird and a rabbit if we are out shooting leads us to believe that the latter will not take to the air. The acquisition of meta-information by recognition is carried to extraordinary lengths of refinement, so that it may be derived from cues so trivial and varied that it would seem incredible were it not commonly observed in the higher animals.

It might appear on the face of it that, where all the subject sees is a spot on a cathode ray tube, recognition would be impossible and certainly to make a choice of tactic would be a hazardous proceeding if the experimenter were trying to defeat the subject in a contest. It is well known, for example, that a simple harmonic input is easily recognized; but it is only recognized after a certain number of cycles and after the habit had been formed of inferring that, given these cycles, the rest of the input would also be this simple harmonic; then if the experimenter designed his inputs so that at this point they changed to something else, the subject would be caught out. In the language of the 'Theory of Games' the subject has to choose between, say, Bayes and minimax

criteria for his choice of strategy. The important consequence of successful recognition is that it permits 'total' prediction and completely alters the tactical possibilities. However, it does not appear to require a great complexity of input (e.g. a compound harmonic target) to make this procedure on the part of the subject very difficult; if suitable noise generators are used to produce the input, it becomes impossible. Nevertheless there are signs that, with considerable replication, a certain amount of recognition is possible with patterns previously thought to be unrecognizable. It is not essential that recognition should lead to wholly predictable results, but good use could be made of partial recognition with the input assigned to a particular class or ensemble.

It is a matter of common experience that auditory patterns of very complex harmonics can be recognized fairly readily, whereas visual recognition is much more acute in relation to complex time invariant patterns. There are some grounds for belief that a simultaneous presentation of an input both orally and visually might be advantageous.

While there may be some cases in which a noise generated input is representative of the kind of task which the subject might have to deal with in real life such, for example, as controlling an aeroplane in gusty weather, there is some doubt as to whether noise generated inputs, insofar as they deny to a subject the possibility of using meta-information, are a good guide to optimizing in the case of real situations where total or partial recognition is to be expected.

The time average of the squared deviation is only a meaningful statistic in relation to the steady state, and therefore, in each individual experimental run, the first part of the subject's behaviour is cut off up to a point where the effect of the starting conditions has died away sufficiently to be submerged in the noise. In the experiments described in Part II this may mean a period up to about 5 to 10 seconds. This rejected part is not, however, without its own importance, since it may represent a substantial proportion of the time during which information is being supplied to the subject. There is no reason to believe that a linear model which may be found adequate for the stationary part of the sequence will be satisfactory for that part which is substantially influenced by initial conditions. If the initial deviations are large there is no necessity for the gain to be limited by stability considerations, and there is some reason to believe that maximum gain is employed, being reduced to the value appropriate to the steady state as the starting transient dies away. It is improbable that in any practical tasks the average squared deviation in this part of the sequence is of great value. In most cases what is important is to reduce the large starting deviations as quickly as possible.

In Part II, Lomnicki and Zaremba describe some experiments bearing on the learning process and its steady state, using the mean squared deviation as a measure. The primary purpose of this work was to throw light on the design of experiments to be conducted with the apparatus, but some useful inferences may be drawn from it. Firstly, there is evidence that, even if behaviour reaches a stationary state, the stochastic process formally describing it would not be ergodic. In this context, non-ergodicity simply means that the subjects have 'on-days and off-days' so that the variation in the measure of their performance is much greater than that which could arise in samples of a stationary

ergodic process. This seems quite in accord with common experience of human behaviour; it does, however, have the effect of requiring a much larger amount of experimental data.

It has already been pointed out that the basic noise arises from error in the quantitative appreciation of the input sequences and in carrying out the intended sequence of handle movements; it is common experience that the magnitude of the basic noise arising from these sources can be modified by changing display-magnification or by changing the sensitivity, i.e. the functor relation between the handle movement and the output. If the basic noise variance did not change with these functor changes, then increase of magnification and reduction of sensitivity would each decrease this variance indefinitely. If on the other hand the basic error were proportional to the observed quantity or required movement (cf. the so-called Weber–Fechner Law) then the functor changes would have no effect on the basic noise variance. In fact, either too great or too little sensitivity increases the noise, as does too little or too great magnification. The experiments relate for the most part to varying the sensitivity μ and it has been found that, if the regression of the mean squared deviations on μ is assumed to be hyperbolic, the fits obtained are as good as can be expected, having regard to the size of the samples and the non-ergodic character of the process. Each regression applies to a single subject and to a single input, and of course, to a particular machine. We can see at once on logical grounds that if the input scale of a target were doubled

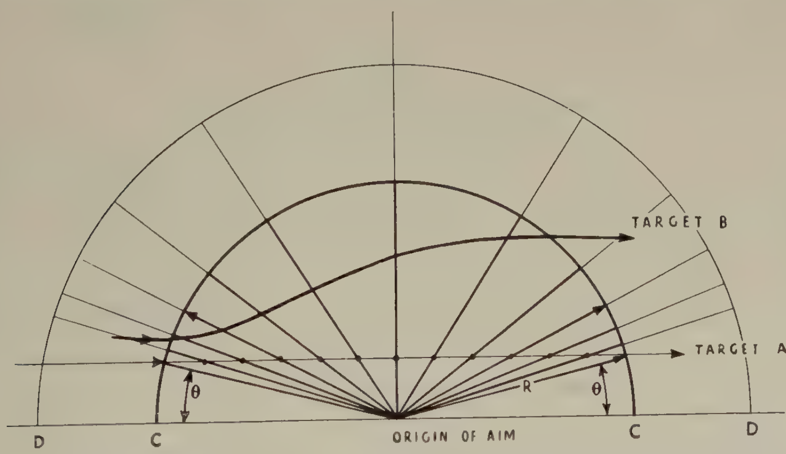

Figure 2. This figure shows how, so far as aiming is concerned, any target motion with varying range is equivalent to another target motion at constant range. It also illustrates for any target motion $\{R(t), \theta(t)\}$ that the aiming error at the target, say, $R\Delta\theta$ will be independent of range if the magnification and sensitivity of the aiming device be appropriately adjusted.

it would be necessary to increase the sensitivity. Indeed, if the display magnification were at the same time halved, no change whatever would be apparent to the subject. Sensitivity and display magnification inappropriate to the scale of the input is one of the most common sources of high basic noise; as a consequence, even within a single input sequence, better results might be obtained if the sensitivity and the display magnification could be appropriately adjusted. For example, in the well known cross-over target (see Fig. 2), the

angular velocity subtended at the origin of aim often starts from something very small, reaches a very high maximum as it passes through the normal of the target path at the point of aim, and then falls off again. In this case considerable improvement should be effected if the sensitivity and magnification could be appropriately adjusted during the run. If information is available about the changing range, then some function of this could be used to feed in the necessary adjustments. This would still hold even if the target path were not linear. It is fortunate that the optima are not critical, so that an accurate solution is not required.

It is possible from the regressions to gain some idea of the extent to which the subject's inability to make accurate handle movements contributes to the mean square deviation.

Coefficients of the hyperbola are shown in the relation

$$E\left[\frac{1}{n}\sum_{i=1}^{n} z_i^2\right] = A\mu + B\mu^{-1} + C$$

so that if B were zero then for $\mu = 0$ the mean square deviation would be equal to C.

Making the highly plausible assumption of the independence of the visual and muscular basic noise, C is the sum of the squared 'bias' and the expected contribution to the realized noise by the visual error in judging the display. Hence the expected contribution due to handle movement errors would be $2\sqrt{(AB)}$.

Table 1 is a table of results rounded off to four significant figures.

Table 1
The Coefficients of the Regression of S^2 on μ

Subject	A	B	C	$2\sqrt{(AB)}$	$C+2\sqrt{(AB)}$	$\frac{2\sqrt{(AB)}}{C}$	Average Score in 10 trials
B1	0·5566	202 250	2761	671	3432	0·2431	4286
B2	1·3870	250 170	3996	1178	5174	0·2948	6030
B3	2·0866	85 720	3466	846	4312	0·2441	5417
B4	0·7225	248 120	4758	847	5605	0·1780	6661
B5	1·6467	97 270	3183	800	3983	0·2515	4615
B6	1·8796	164 290	3411	1111	4522	0·3258	5721

Since the values of A, B and C depend not only on the *basic* noise level but also on the effect of strategy (e.g. the transformation function) employed by the subject the ratio $2\sqrt{(AB)}/C$ is more useful for inter-subject comparison.

It must be remembered that the measures are all squared errors, which tend to exaggerate differences in the partition of, say, the 'probable error'.

It is hoped to try similar regressions for display magnifications. Should these be successful (and there are some experimental difficulties) the resultant hyperbolic surface should make it possible to isolate the two components of C.

It will be noted that, throughout, sequences have been employed rather than functions. Although the assumption of continuity in the mathematical sense has many conveniences, much more is known about discrete stochastic models than continuous ones. It should be clearly understood, however, that it is meaningless to ask what happens in the 'interval'. Mathematical continuity is not physically realizable, since it implies an infinite amount of information. A graph contains less information than the table from which it is

constructed; its usefulness is as an aid to an insight into relation patterns. Similarly, so-called continuous recording instruments are not continuous in the mathematical sense. Fairthorne (private communication) has suggested that such records should be described as 'blurred' representations, as they contain no more information than that contained in tables constructed from them less the inevitable equivocation.

PART II

§ 1. Introduction

The experiments described below were concerned with the performance of compensatory tasks, i.e. tasks in which the operator can see only the *difference* between his output and the input he is required to match. Tracking an aircraft seen against a clear sky is a typical task of this kind, and indeed the study of the performance of compensatory tasks was initiated during the last war for the purpose of optimizing the design of gun turrets in aeroplanes. There are many other important cases where the task of a human operator is purely compensatory, for instance blind landing of an aircraft by means of instruments. However, it is convenient to refer to the input as a target, and to describe the task as tracking.

§ 2. Apparatus

For the sake of simplicity, it was decided to begin with one-dimensional tracking; an extension of the equipment providing for the simulation of two-dimensional tasks would present no difficulty from a technical view point. The apparatus, which was designed by H. J. Clark of Boulton Paul Aircraft, Ltd., computes the mean error and the mean squared error of the operator. The latter can be regarded as an overall figure of merit for the performance of any particular task, though it is fully realized that any such choice is logically arbitrary and does not necessarily correspond to the practical requirements of a given instance of a compensatory task. The mean error indicates only the bias. Thus the equipment can be divided into the following sections: target simulator, tracking control, analogue computor, display timing control, and error measurement.

As it is well known that individuals vary widely in their performance, and as a wide variety of targets would be needed, an electric analogue computor was chosen. This choice provided a ready means of altering target velocities, tracking control sensitivity, display magnitude and error measurement sensitivity.

The analogue computor is fed with voltages from the target simulator and from the tracking control. The latter is divided into two parts, one representing the position of the tracking mechanism, and one the velocity. By this means either pure position control or pure velocity control or aided-lay with any ratio of velocity to position can be obtained.

The target is generated as a velocity; the integration required to generate the target position then effectively smooths out any small high-frequency terms generated in the target simulator. The voltages representing target velocity and tracking velocity are subtracted and the difference applied to an electronic integrator. The voltage output of the integrator then represents the

relative position of the target if pure velocity control is being used. If position control or aided-lay is being used, a further correction has to be made, and this is done by subtracting the tracking control position voltage from the integrator output. The resultant voltage then represents the tracking error. This error is amplified and displayed as a displacement of the spot on a cathode ray tube. The task of the operator is to keep the spot in line with a graticule.

The error measurement is made by integrating and accumulating the error. The error is also fed into a squaring circuit, and the accumulated square of the error is recorded. Provision is made for taking pen tracings of any two voltages in the equipment; the ones normally recorded are handle movement and error.

The accuracy of the simulator is of the order of 1 per cent of the maximum output, and the accuracy of the error measurement is also approximately 1 per cent of the maximum error. As some operators are much better than others, arrangements are made so that the error voltage may be amplified before being measured. This improves the accuracy at low values of error.

§ 3. Procedure

A series of experiments were carried out for the purpose of studying simultaneously the learning process and the effect of changes of sensitivity in the controls. The subjects, six works apprentices, here denoted by B1, B2, B3, B4, B5, B6, were made to track a compound harmonic target the velocity of which, measured in inches per second, was given by the formula:

$$\frac{1}{4}\left(\cos\frac{2\pi t}{30}+\frac{3}{2}\cos\frac{2\pi t}{10}+\frac{3}{2}\cos\frac{2\pi t}{5}\right),$$

in which t represents time in seconds.

Velocity control was used throughout. Theoretically, one can conceive of a full range of sensitivities from 0 to infinity; practically, difficulties arise from the limited range of handle movements and from the finite size of the display screen. Thus, the lowest possible sensitivity was defined by the condition that the maximum displacement of the handle should match the greatest input velocity. The upper bound of the sensitivity range was less precisely defined, and depended on the operator rather than on the target; it corresponded to a sensitivity with which a very slight movement of the handle caused the spot to flash from one side of the screen to the other, so that keeping it displayed became hardly possible. The sensitivity range was explored from the lowest sensitivity compatible with the given target to one where 'catching' the spot was beginning to be uncomfortably difficult. More precisely, the sensitivities, measured in thousandths of an inch (mils) per second per degree of handle displacement, were:

50, 75, 125, 200, 300, 500, 750, 1250, 1750, 2500.

Each of the 21 trials consisted of one run with each of these sensitivities, i.e. ten runs in all. In order to average out sequential effects, the permutation of these sensitivities was chosen at random at the beginning of each set of trials with the six subjects. The recorded part of each run lasted 30 seconds, which is the period of the target. Before the counting of the mean squared and mean errors started, the subject was given 10 seconds to adapt himself to the sensitivity, and to overcome the effect of the starting conditions; this relatively short amount of time proved to be quite sufficient for the purpose. The

instructions given in the Appendix were a guide for the experimenter, who explained their gist to each subject and made sure that they were properly understood.

§ 4. Results

The score used to assess a subject's performance in any particular trial was the mean, over all the runs, of his squared error, measured in squared mils. Quite apart from a very obvious downward trend during the first half of the experiment, there were fairly considerable variations in scores from day to day, this being observed also in a number of pilot experiments. In view of the small sample size, this scatter made attempts at fitting learning curves rather pointless; however, the general impression was that learning proceeded by a sequence of jumps from one quasi-steady state to another, presumably due to changes in the strategies of the subjects. Results for two subjects are shown in Fig. 3.

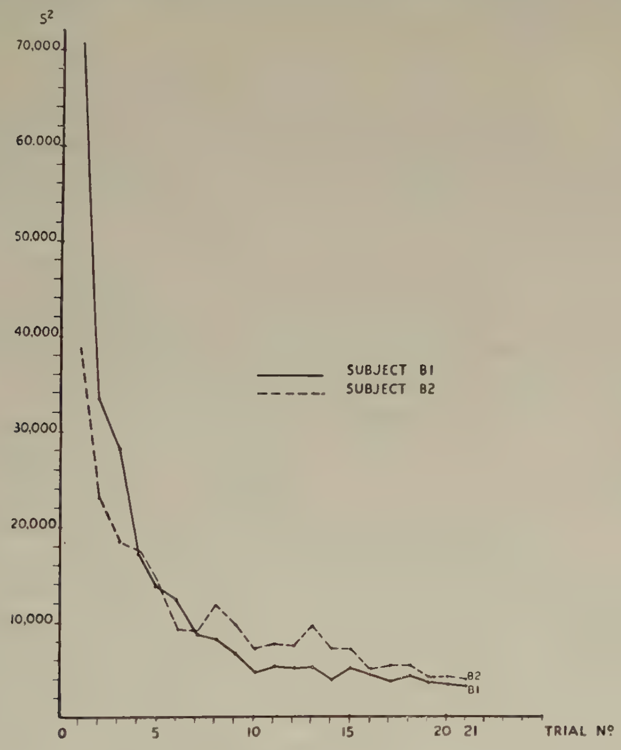

Figure 3. Results of two subjects showing progress with learning.

A surprising result of the experiments was the appearance of an appreciable negative correlation between the scores obtained at the first trials and those achieved after a sufficient period of training. The pertinent facts are given in Table 2.

The rank correlation coefficient (Spearman's ρ) between the initial score and of the average over the last 10 trials is equal to $-0{\cdot}6$. Although it does not in the present sample reach the normally accepted criterion of significance (P in the present case is about $0{\cdot}1$), a number of pilot experiments gave results of the same kind. Whether or not a larger sample would confirm the negative correlation, a substantial positive one seems unlikely. This result has an obvious

implication for the selection of operators—good initial score is a poor predictor of good score after practice. It is hoped that further experiments will elucidate this point more thoroughly. Meanwhile, the conclusions discussed above should be treated cautiously; in particular, it should be borne in mind that the group in question could be regarded as belonging to a fairly homogeneous population.

Table 2
Changes of Score with Learning

Subject	B1	B2	B3	B4	B5	B6
Initial score	70682	38805	43757	49960	51502	53515
Best score	3240	4001	3720	5017	3108	4101
Achieved in trial No.	21	21	19	17	18	20
$100 \times \frac{\text{Best score}}{\text{Initial score}}$	4·58	10·31	8·50	10·04	6·03	7·66
Average score in the last 10 trials	4286	6030	5417	6661	4615	5721
The same in hundredths of the initial score	6·06	15·54	12·38	13·33	8·96	10·69

As could be expected, the scores increased considerably when the control sensitivity became uncomfortably high, or low. In trying to fit a regression curve of the score against sensitivity, a function had to be selected which would tend to infinity both when the sensitivity tends to 0 and when it tends to infinity. The simplest expression satisfying this requirement is

$$S^2 = A\mu + B\mu^{-1} + C,$$

where S^2 is the score, and μ is the sensitivity of the controls. The results of the fitting, based on the mean scores for each sensitivity during the last 10 trials are given in Table 1. An expression of the type

$$S^2 = A\mu^2 + B\mu^{-2} + C$$

was also fitted; the two fittings will be referred to as regressions of S^2 on μ and μ^2 respectively.

A thorough statistical interpretation of these fittings presents serious difficulties, which are greatly enhanced by the uncertainty concerning the statistical structure of an appropriate model. In any case, the smallness of the sample does not seem to warrant a considerable effort in that direction. Nevertheless, some indications concerning the acceptability of either of the two fittings were required. The estimation of the variances of the scores for each individual and each sensitivity would have been an obvious first step in that direction. The sample size made an accurate estimation of the 60 variances impossible *a priori*, and indeed the 60 sample variances showed considerable fluctuations from sensitivity to sensitivity and from person to person. However, an inspection of the table of these sample variances suggested that their variation with sensitivity was predominantly spurious, while differences between individuals appeared to be significant. Therefore, for each individual, the sample score variances were averaged over the 10 sensitivities. The values thus found for the standard errors of the subjects B1, B2, B3, B4, B5 and B6 were respectively 381, 796, 667, 835, 542 and 650 mils. squared. A comparison with the overall average scores in the last 10 trials given in Table 2 shows, incidentally, that the two sets of magnitudes are highly correlated; the rank correlation coefficient is 0·943, which is unquestionably significant.

On this basis it was possible to compare the fit of the two regressions. In Table 3, the first column corresponding to either regression contains the sum of the ten squared deviations for each of the six subjects, while the second column shows the maximum absolute deviation for each individual; these figures are normalized by being divided by the estimated values of var S^2 in the first case, and of $\sqrt{(\text{var } S^2)}$ in the second. The figures in the last line represent the totals of the normalized squared deviations and can be regarded as overall measures of fit for each of the two forms of regression.

Table 3
The deviations in the Two Regressions

Subject	Regression of S^2 on μ		Regression of S^2 on μ^2	
B1	4·002	1·204	11·951	2·027
B2	1·776	0·862	1·707	0·754
B3	5·682	1·488	9·003	1·699
B4	2·810	1·226	2·908	1·366
B5	10·918	1·544	17·095	2·250
B6	5·489	1·522	6·658	1·456
Total	30·677		49·322	

The fit of the regression of S^2 on μ is as good as could be expected, and clearly better than that of S^2 on μ^2. By way of illustration, the regressions of S^2 on μ for the subjects B1 and B2 are shown on Figs. 4 and 5; the asymptotes of the fitted hyperbolae, as well as the points of (fitted) minimum score, i.e. the points $\mu=\sqrt{(B/V)}$, $S^2=C+2\sqrt{(AB)}$ are also plotted.

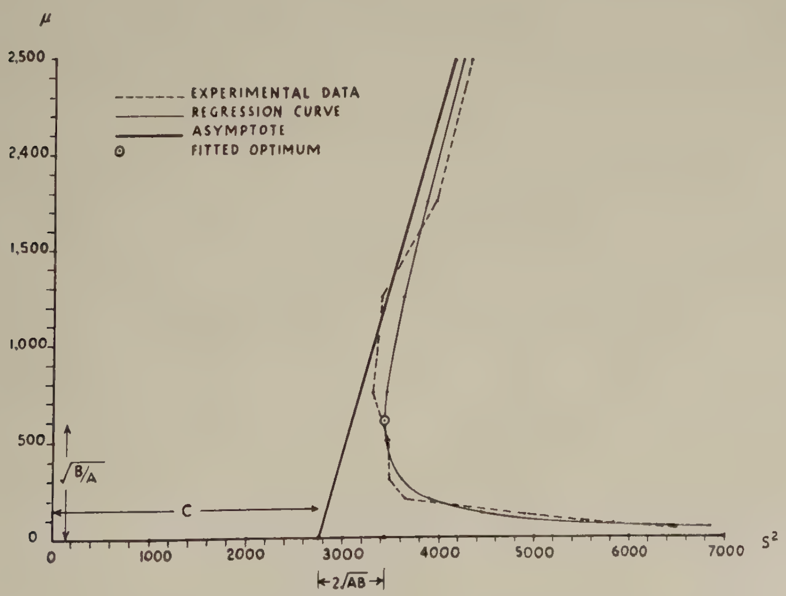

Figure 4. Regression of S^2 on μ for Subject B1.

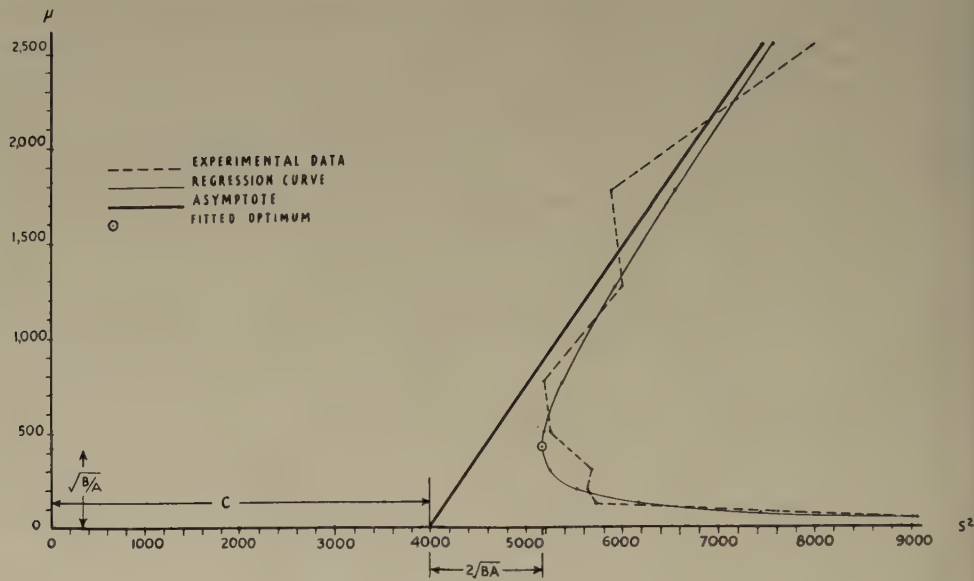

Figure 5. Regression of $\grave{S}^2$ on μ for Subject B2.

Appendix

Instructions to Operators

Here we have a machine which will be able to measure your ability to perform an important kind of precision task in which your action is based solely on a knowledge of the error you are committing. Examples of such tasks are the tracking of an aircraft in a clear sky, or blind landing of an aircraft on instruments.

On the screen you will see a fixed black mark which represents the direction of your aim, and a moving green line which represents the target. The distance between these two lines shows how far off the target you are aiming, in other words the error which you are committing. You will be expected to follow the target for a fixed period, keeping the distance between the black and green lines as small as you can. *To achieve this you will have to turn the control handle in the direction in which the green line deviates from the black.* (Demonstration.)

The machine will measure your overall performance by recording the total squared error, and after each run you will be told your score; the better you become, the smaller will be your score. It will also be shown whether you are inclined to aim more to the left or to the right of the target.

Each day, over a period of four to six weeks, you will make a trial consisting of ten runs. You will notice that the task varies in each run; in some tasks very small movements of the handle will be sufficient to reduce the error, in other cases quite large displacements of the handle will be necessary. We shall refer to 'high' and 'low' handle sensitivity respectively. Every run will last for 40 seconds but the errors during the first 10 seconds will not be included in your score. This initial period is intended to give you time to adjust yourself to the particular task and to overcome the initial error.

Operators differ in their ability to perform tasks of this character. Some have accurate perception, quick responses and the ability to make precise movements. Others may not have the initial ability but a greater capacity to improve their performance by learning. The machine was constructed to enable these factors to be investigated and you have been chosen as a representative group to take part in this experiment. Our only requirement is that you carry out the trials to the best of your ability.

La I[e] partie est une exposition générale de quelques facteurs qui doivent être pris en considération dans le projet et l'interprétation des expériences du contrôle humain. En particulier le caractère 'non-ergodique' de l'état asymptote de connaissances (pseudo-equilibre statistique) demande des précautions spéciales et une grande quantité de données. L'attention est attiré sur le rôle joué par les types differents de la variation.

La II[e] partie est un compte-rendu de certaines expériences faite avec une machine incorporant des éléments à computer, afin que des données statistiques peuvent s'accumuler rapidement *inter alia* en forme de carrés moyens des deviations. Les expériences décrites se rattachent aux phases diverses de connaissances et la régression de carré moyen des deviations sur la sensibilité dans l'état asymptote.

Teil I bildet eine allgemeine Darstellung einiger Faktoren die in Betracht gezogen werden müssen bei der Planung und Erklärung von menschlichen Kontroll-Versuchen. Insbesondere verlangt der night-ergodische Charakter der assymptotischen Anlernperiode (statistisches Quasi-Gleichgewicht) eine besondere Vorsicht und eine grosse Anzahl Beobachtungswerte. Die Aufmerksamkeit wird auf die Rolle von systematischen und zufalligen Variationen gelenkt.

Teil II enthält einen Bericht über gewisse Versuche mit einer Maschine die Elemente einer Rechenmaschine enthält so dass eine schnelle Sammlung von statistischen Ergebnissen möglich wird, u.a. in Form der mittleren Abweichungsquadrate. Die geschilderten Versuche erstreckten sich auf die verschiedenen Stufen des Anlernens und auf die Regression der mittleren Abweichungsquadrate auf die Empfindlichkeit im 'assymptotischen' Zustand.

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PHYSIOLOGICAL MEASUREMENTS AS A BASIS OF WORK ORGANIZATION IN INDUSTRY

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The measurement of the work-load in industry necessitates the use of reliable methods and of apparatus which is both robust and foolproof. Three methods are usually employed at our Institute: (1) an exact time-study covering the whole working day and including details of all accessory operations and all rest pauses; (2) the measurement of energy cost of work by the use of the respirometer developed at the Institute; (3) the measurement of pulse rate by means of the Müller pulse counter.

A number of examples are quoted of the use of these methods in several forging shops and in a motor car factory. The examples include instances of underloading and overloading of the worker, and the reasons and remedies are discussed.

One of the most important problems of industrial physiology is to find out whether a man's work may be considered as normal or whether it means an overload or underload of the man. It is evident that for this purpose it is not sufficient to make short-period measurements of the most typical elements of the work in question. We should try to get a true figure for the whole course of the work including all accessory elements and all rest pauses, even the shortest. The primary question is: What methods are at hand to work successfully in this field? It is clear that only such methods can be used which are applicable during the work itself and which do not, or at least do not significantly, influence the normal course of the work. This target is the more difficult to reach the more we are dealing with mental work and lighter forms of bodily work, because the normal rest pauses which can be used for measuring purposes are shorter and rarer than with heavy work, and because these types of work are disturbed by influences other than those which affect heavy bodily work.

A survey of the methods at hand for this purpose shows that their number is very small. Only those methods and instruments can be used which have reached a certain degree of reliability and are easy to handle in the factory environment, i.e. a certain robustness of construction is called for. Besides supplementary measurements varying from one case to another, in our Institute we usually employ three fundamental methods.

The first method is not really a physiological one. We have taken it over from the engineers. It is an exact time-study. For our purposes, it is necessary to make this time-study throughout a full working-day. If possible we carry through the time-study not only over one but over two or three full days for each man and make it as detailed as possible.

For instance, it is not sufficient to determine how long a man is occupied with a certain type of work, but it is necessary to discover how much time is used for every single work-piece, for every accessory operation, and above all for every rest pause. With some types of work it is necessary to record rest pauses down to one or two seconds; with others it is sufficient to go down to

ten or twenty seconds. It is necessary to distinguish whether these rest pauses are voluntary or whether they are waiting-times over which the worker has no control. It is not easy to make such time-studies lasting many hours, and not rarely is it more strenuous work recording the times than doing the work under examination!

The second method we use—except for lightest forms of work and mental work—is the measurement of the energy cost of the work. For this we use the respiration gas-meter developed in our Institute which measures the volume of the expired air and automatically takes a sample of this air for subsequent analysis. Unfortunately, even with this apparatus, it is not possible to make measurements over full working-days; tests of ten to twenty minutes' duration can be made, however, without any appreciable interruption of the work, and nearly without disturbance of the normal course of work.

These metabolic measurements give a good indication of the heaviness of the bodily work performed. We define the 'heaviness of work' as the caloric output during working-time minus the basal metabolism. In order to obtain an idea as to whether a man is overloaded or underloaded in regard to energy output, the following considerations have proved very useful and in good accordance with practical needs.

We start from the point that the maximum energetic output a normal man can afford in the long run, without any damage to his health or premature decrease of his strength, seems to be about 4800 kcal per day. This figure cannot be found by experiment, but it agrees fairly well with the assumption of the nutritionists in all countries and with the findings of work physiologists who have made metabolic measurements on different types of work. There is no doubt that extremely strong men may reach higher values and that even men of medium strength may reach higher values over weeks or months. But, on the yearly average, 4800 kcal per day is a limit that should not be exceeded.

A man's basal metabolism over 24 hours plus the calories he needs for personal purposes in leisure time, including eating, dressing, washing and going to work, yields a figure of 2300 kcal per day. This means that the maximum work-calories at disposal for one working-day—usually 8 hours—are 2500.

With industrial work, the interest is not only in maximum caloric values but even more in values which can be taken as standards; the present example is a standard for heavy workers. If 2500 kcal are the maximum, 2000 kcal must be a value which might be said to be a normal. Consequently, if a heavy worker expends 2000 kcal per day working, we can call this a suitable load for him. If the work is done at the same rate throughout the working day of 8 hours, the work-calories should be 250 per hour or 4·2 per minute, as an average over the whole working time. For practical purposes it will be sufficient to take 4 kcal per minute as an average figure for the whole working-day of 8 hours, and this average should not be exceeded.

Many types of work demand a caloric output per minute—basal metabolism being subtracted— higher than 4 kcal, e.g. 6, 8, 10 kcal or even more. It seems reasonable to calculate from these figures what rest pauses a man must have to reduce the average work-calories per minute, taken over the whole work period, to a figure of 4 kcal. If, for instance, the work requires 8 kcal/min, he really should not work more than one half of the whole working-time. This means—

expressed in terms of the time-study system—a fatigue allowance or compensating rest of 100 per cent. Usually the allowance is expressed as a percentage of the real working-time, not of the legal or paid one. To find the necessary fatigue allowance in case of heavy bodily work we may use the formula:

$$\mathrm{F.A.} = \left(\frac{\mathrm{kcal/min}}{4} - 1\right) 100.$$

We have very often had the opportunity to compare the results of this calculation with practical experience, and have found good agreement in cases of heavy bodily work without heat stress or other effects, such as a large amount of static work or high level of attention needed. The fatigue allowance calculated by the caloric output is a minimal one and has to be increased if any other stress is present. It is of course hardly necessary to point out that the calculation is useful only when the working-calories per minute are greater than 4.

The third method we use is the measurement of the pulse rate continuously over the whole working-day. This is possible now by using E.A. Müller's electronic pulse counter which has the great advantage of not influencing the normal course of work and not disturbing the worker in any way. The heart rate is correlated with the caloric output, but the regression coefficient is an individual one and depends on the strength of the man in question. The more efficient the man is the smaller is the coefficient. The heart rate is increased by high environmental temperature and by muscular fatigue, beyond the level given by the metabolic output and the individual factor. The effect of heat on the pulse curve being very impressive, it is in practice often much easier to estimate heat stress by measuring the effect on the heart rate than by making physical measurements. This is especially so when the body is exposed to a large amount of radiant heat varying in intensity and duration. Muscular fatigue, too, makes the pulse rate rise beyond its normal value. Temperature conditions and caloric output remaining constant, a rising pulse level over the working-day is a significant indication of increasing fatigue. A high pulse rate in connection with low caloric output means fatigue of some muscle groups, especially by static work.

The following illustrations demonstrate the consequences of changes in the work organization in several different factories. Figure 1 shows curves taken in a drop forge where relatively small pieces weighing 0·55 kg each are handled. The upper curve shows the time distribution in per cent of the whole working-day. The black part means the real forging-time, i.e. about 63 per cent. The next part gives the time required to start and end the work. The next 'accessory work' seems rather long in this case. That time was needed to fix the die. Next comes heating pauses required to heat the iron for forging. These pauses mean also recovery pauses for the man. The time for voluntary pauses is very short here. The last are the legal pauses. The graph at the bottom of the figure shows how these different times are distributed over the day. It should be noted that the rewarming-pauses interrupt the course of work rather regularly. The intervals of these pauses would have been even more regular if refixing of the die had not been necessary.

The second graph from above represents the number of work-pieces per hour. Here the regularity is not very great. The next curve is the caloric

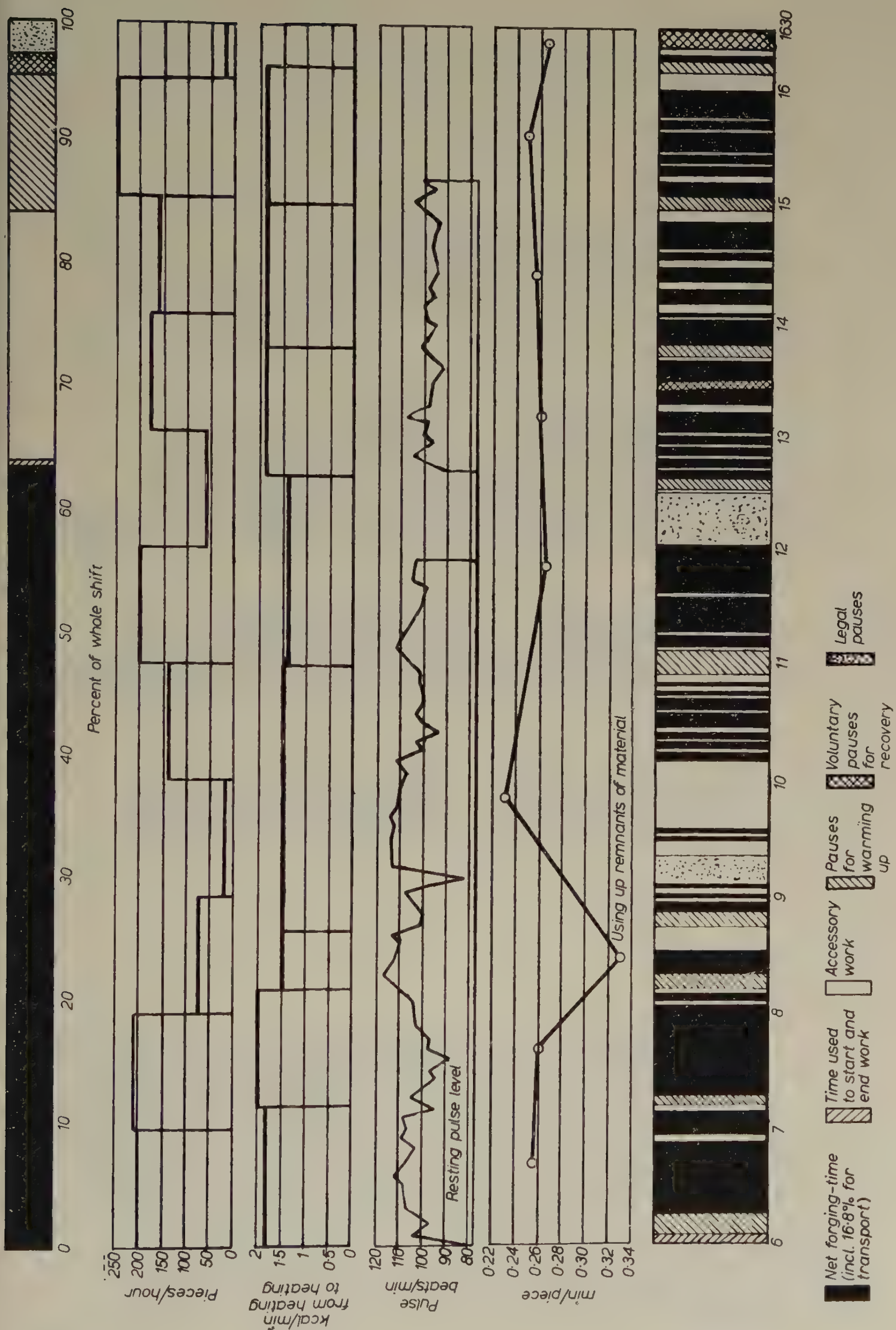

Figure 1. Drop smith forging light pieces of 0·55 kg each.

output per minute. It is interesting to see that this graph is much more regular than the previous one. This depends on the heaviness of work in refixing the die and shows that the distribution of work over the day, which seemed irregular when judged by the output graph, is automatically regulated on account of physiological needs. On the whole, the caloric output is 1·5 to 2 kcal/min, or about half the level tolerable over the day.

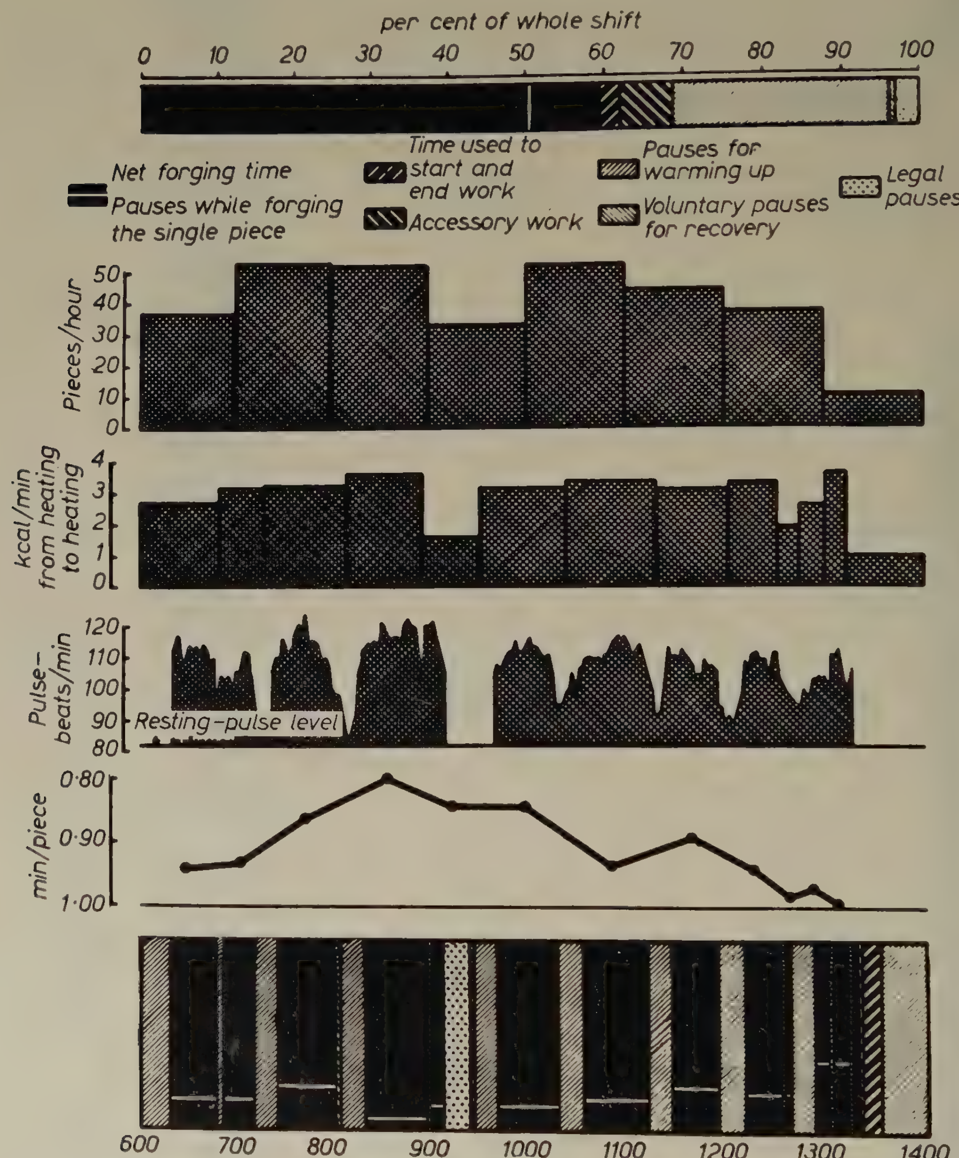

Figure 2. Drop smith making pieces of weight 4·8 kg.

The next curve is the pulse record. It shows no sign of increasing fatigue but, on the contrary, decreases in the afternoon in spite of higher output and rather high caloric consumption. This depends apparently on the accessory

work. Fixing the die means much static work. As a whole, the pulse curve is higher than would correspond to the caloric output. In this case it is due to both a certain amount of static work and the influence of heat. It is obvious that during the heating-pauses the pulse level does not return to basal value. This is so because the man remains under heat influence during the pauses, these pauses being not more than a few minutes.

The man seems to make no voluntary pauses. This indicates that he himself thinks the heating-pauses sufficient for his own recovery. We also find no

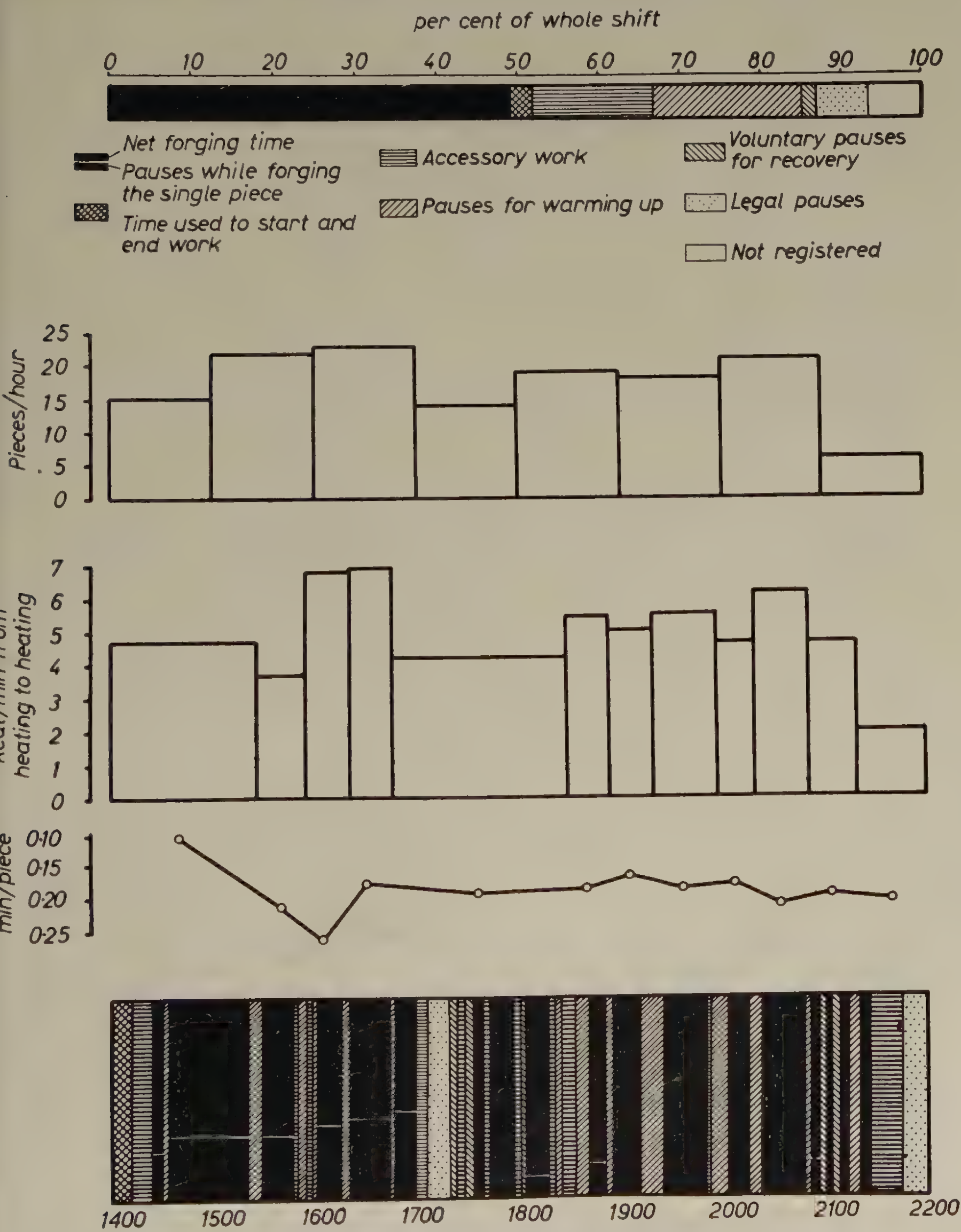

Figure 3. Forging heavy work-pieces (32 kg each) with a very heavy hammer.

indication that this man is overloaded. On the other hand, his pauses should not be shorter than they are because of the rather high pulse-level. This means that the heating-pauses which are said to be made for technical reasons are distributed fairly well and are in good accordance with the physiological needs. That's what we find rather often in these types of forges. It is very probable that the length of these heating-pauses is not only due to technical reasons but is also influenced by the workers according to the recovery they feel they need.

Figure 2 is drawn in the same way. The weight of the pieces is 4·8 kg and the actual forging-time about 60 per cent of the working day. About 10 per cent of the time is occupied by very short pauses during forging, during which the man holds his tongs. These pauses are therefore of reduced value for recovery. Furthermore, the man has about 30 per cent real rest pauses. The largest part of these are rewarming-pauses. The man reaches his maximum output at about 8.30 a.m., slowing down towards the end of the shift. This is certainly not the consequence of fatigue, otherwise he could not have finished his work three quarters of an hour before the actual end of the shift. The caloric output reaches about 3 kcal/min. As a whole, the rest pauses of this man are unnecessarily long. During these pauses he is not influenced by heat. So the pulse level goes down fairly well. The length of the pauses is due to technical reasons.

Figure 3 was taken from a very heavy hammer forge, the weight of the work-piece being 32 kg. The real forging-time is not more than 50 per cent, of which 10 per cent represents intermittent shortest rest pauses. So the actual

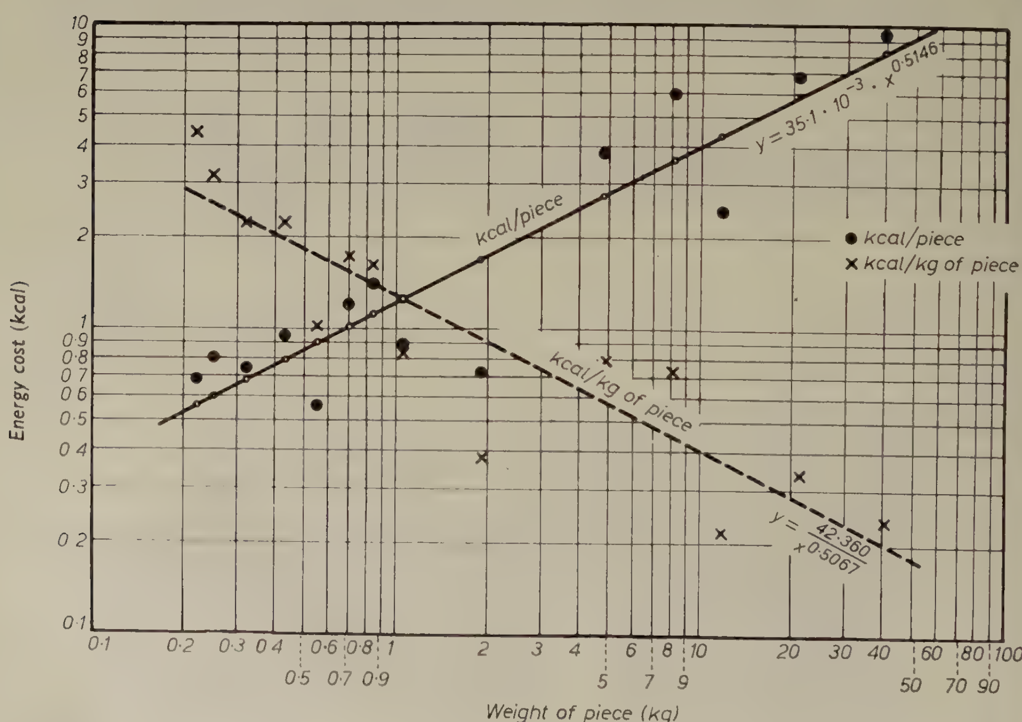

Fig. 4. Calories per work-piece and per kg of work-piece plotted against the weight of work-pieces in a double logarithmic scale.

forging-time is not more than 40 per cent of the shift. In spite of this, the caloric output is about 5 kcal/min on average. This means that the rest pauses would be too short even if there were no heat stress. Unfortunately, no pulse curve was taken, but it is known that the heat stress was considerable. So we must admit that this man is overloaded. This man, the ' first smith ', was the real bottleneck of the whole hammer on which four men worked. The other three were not overloaded. From these findings we could advise the manager to place two ' first smiths ' to this hammer and to make them work alternately. In this way the output of the hammer could be considerably increased and the productivity of the whole process increased despite the one additional wage paid to the newly engaged smith.

Figure 4 shows that the calories per work-piece with very different hammers —the weight of the work-pieces ranging from 0·2 to 40 kg—are approximately on a straight line, if calories as well as the weight of the work-pieces are plotted in a logarithmic scale. As a first approximation the same is found if the

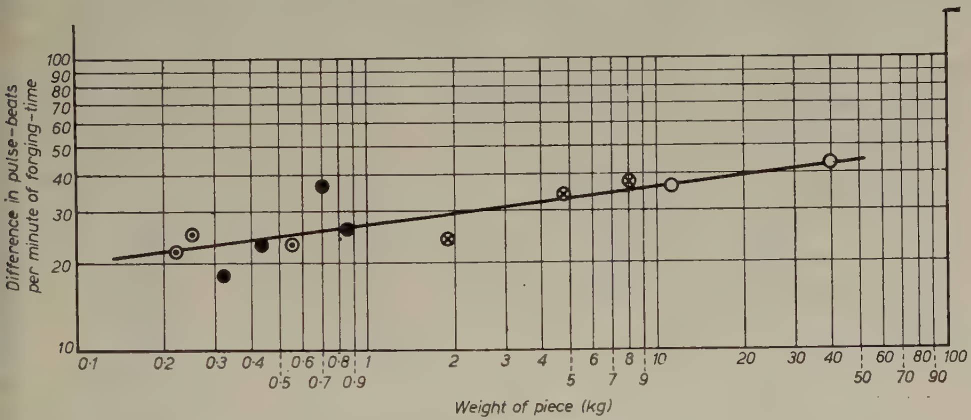

Figure 5. Difference between working-pulse level and resting-pulse level, plotted against the weight of work-pieces (double logarithmic scale), taken in four different factories.

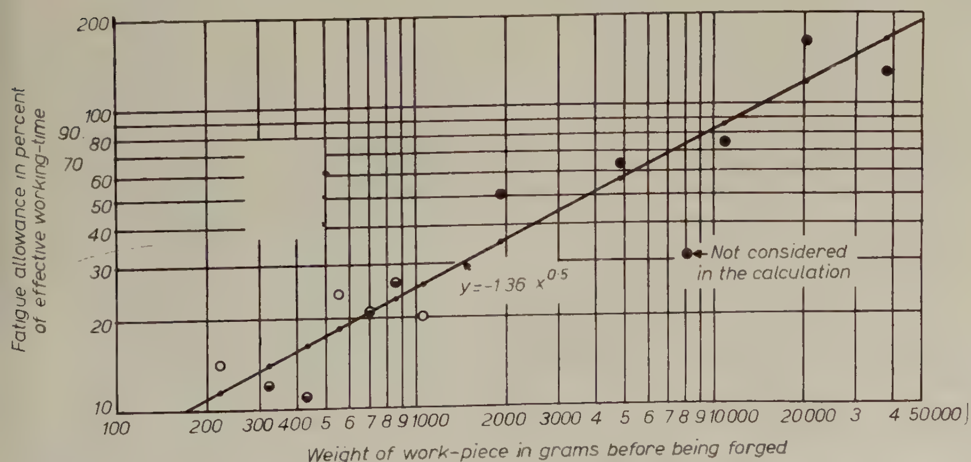

Figure 6. Fatigue allowance plotted against weight of work-pieces (double logarithmic scale), taken in four different factories.

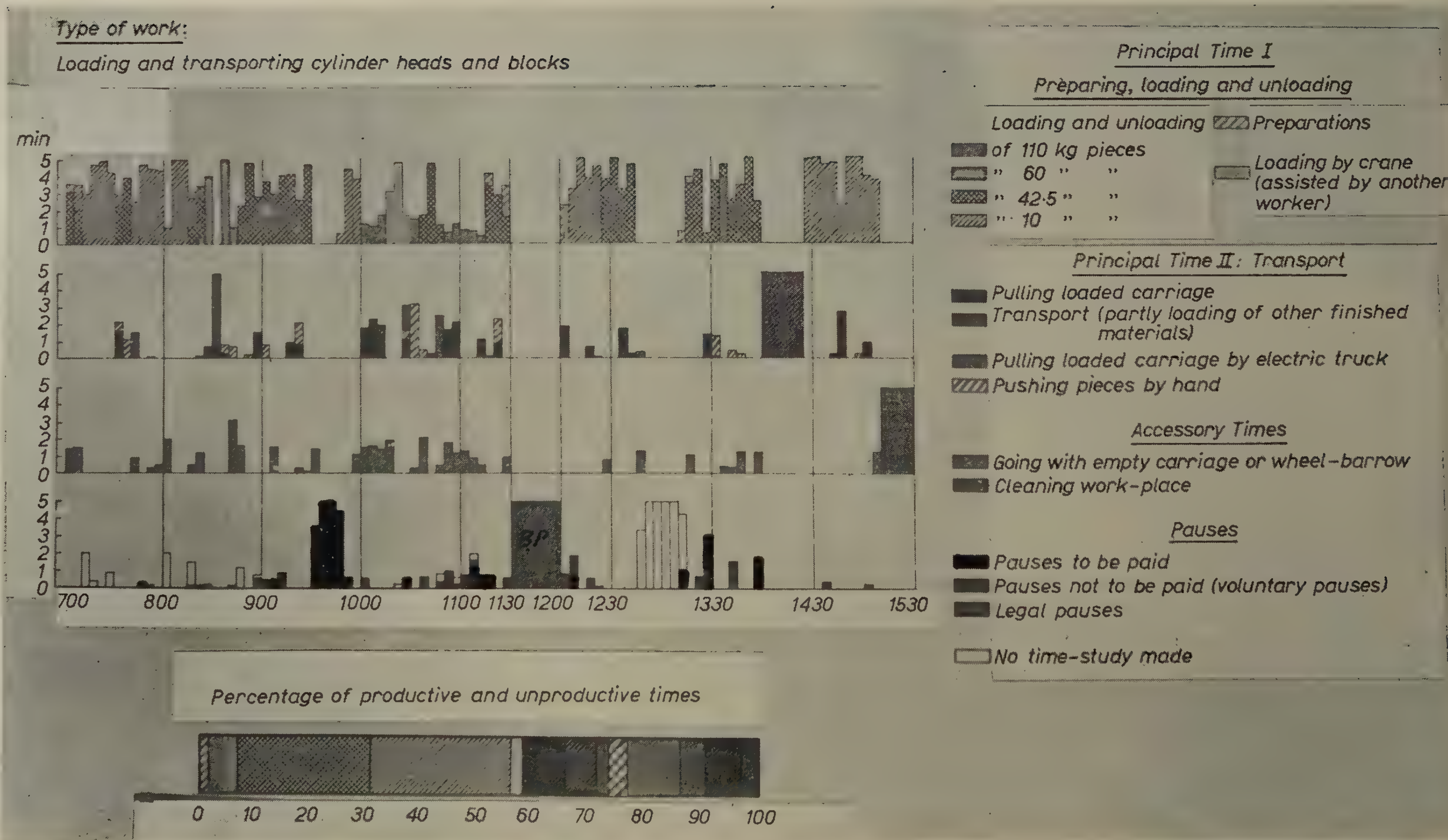

Figure 7. Course of work of a transport-man in a motor-car factory. Note: BP = Betriebs-Pause, i.e. 'Legal' Pause.

pulse difference—i.e. the difference between working- and resting-pulses—is plotted against the weight of the work-piece (Fig. 5). The linearity would be far better if the heat conditions were the same in every instance. If the length of the rest pauses found in all these examples is expressed in terms of fatigue allowance, a fairly straight line is found by plotting the weight of the pieces and the fatigue allowance in a logarithmic scale (Fig. 6). For practical purposes adjustments have to be made, above all because of the different degrees of heat stress. Nevertheless, such a graph gives a good idea to the production engineer of what pauses are physiologically necessary for the worker, and enable him to use his furnaces most efficiently in relation to the needs of the operatives. The results of such investigations emphasise the need to adapt the efficiency of the factory to the working capacity of the man, instead of—as is very often found to-day—forcing man to adapt himself to the technicalities of the machine and its efficiency.

The figures (7 to 14) show curves taken in a motor-car factory. It is a very modern one, the daily output being about 60 to 70 cars. Most of the men were working at a conveyor belt. It is often thought that the steadiness and regularity of work on a conveyor belt is most striking, but time studies over the day showed that the regularity is very limited (Fig. 7). The graphs show the activity of a worker who was not actually working at the conveyor belt but was transporting motor-blocks to the starting-point of the conveyor. It might be expected, according to the timing of the belt, that he would have to do the same thing every 8 minutes. The graph, however, shows that he did very different things in a very irregular way. Every entry on the graph represents 5 minutes' work. About 10 per cent of the whole working-time is occupied with accessory

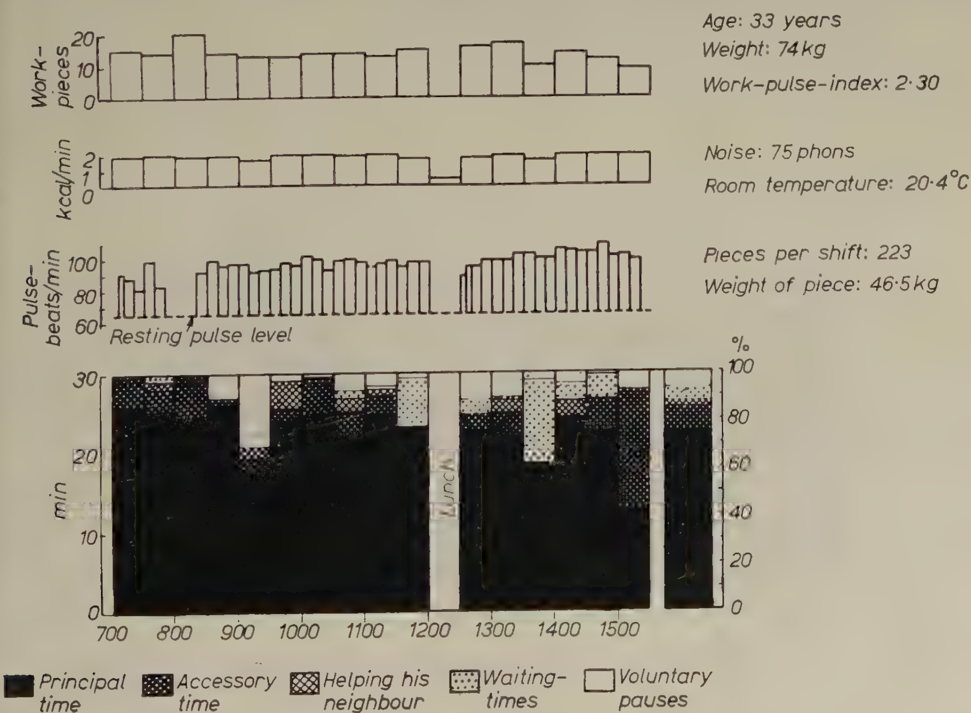

Figure 8. Fixing and securing nuts holding the crank-shaft.

work, but nearly 15 per cent is voluntary rest pause. This means that the total duration of the rest pauses during this 8 hours' shift is about 72 minutes, and that between 9 and 10 a.m. the worker is able to take a rest pause of nearly 15 minutes.

The next man is a worker on the conveyor belt (Fig. 8). His task was to secure the nuts holding the crank-shaft. In the lowest graph the black fields indicate his real work within every half-hour. Here, too, the irregularity is surprisingly large. The dark checked fields represent accessory work of which, in most cases, a certain part is camouflaged rest pause. Crosshatching represents help given to a neighbour who apparently had much less time than our man. The dotted fields are waiting-times and the white ones voluntary pauses.

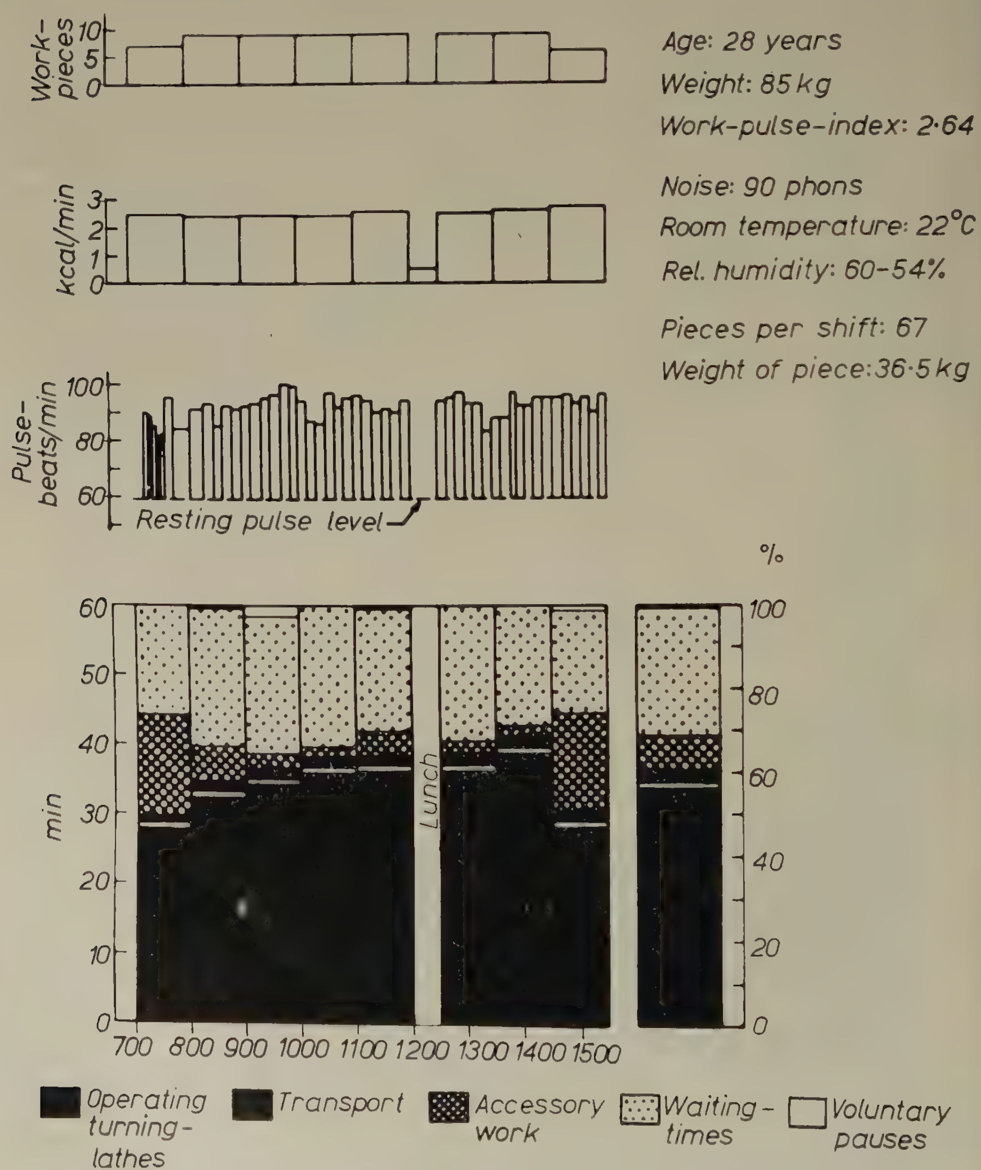

Figure 9. Making crank-shafts on two turning-lathes.

As a total, the pauses amounted to 13 per cent. As to the caloric output, the effort was low, i.e. below 2 kcal/min. Nevertheless, the pulse rate increased during the day. This cannot be the consequence of heat stress. Fixing and securing the nuts means a static effort of the muscles, especially of the forearms. This must be the cause of the rising pulse rate in spite of the decreasing output, and the decreasing actual working-time per half hour. To lengthen rest pauses would certainly not be the right way to help this man. It would be far better to mechanize the process or to improve his tools in order to avoid muscular fatigue caused by static effort of the forearms.

The man from whom Fig. 9 was taken has to operate two turning-lathes at the same time. Alternately he goes from one to the other and has rather long waiting-times of about 30 per cent of his whole shift, but almost no voluntary pauses. The work-pieces—crank-shafts—are rather heavy, about 36·5 kg, and to fix them in the lathes he has to hold the crank-shaft relatively distant

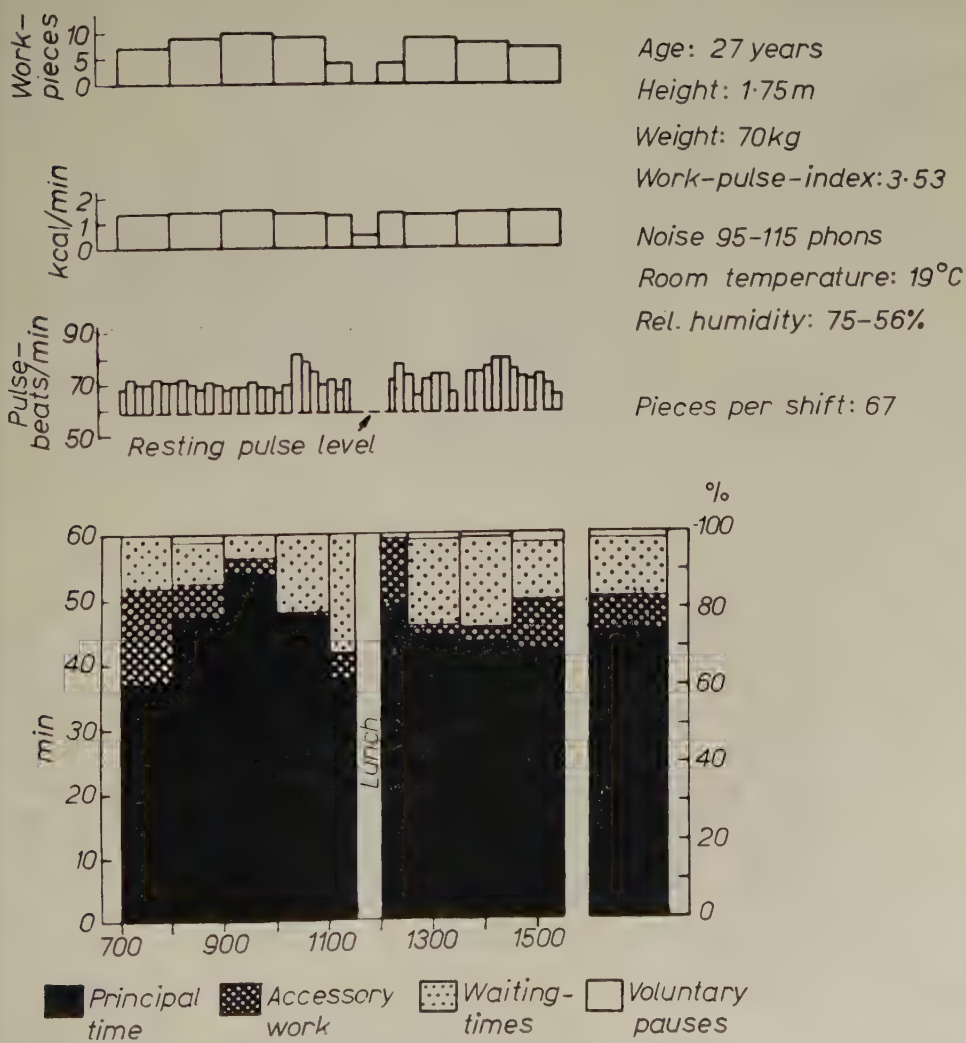

Figure 10. Autogenous welding of car body.

from his body. That is why his average caloric output is rather high, about 2·5 kcal/min, in spite of the fact that he is active not more than 50 per cent of his whole working-time. The pulse rate is increased by the static work. If the worker had not been a very strong man with a work-pulse-index of 2·64, he could not have done this work. The work-pulse-index is the increase of the pulse rate per kg-m/sec. The higher the work-pulse-index, the lower the strength of the man. The lathes in use were very old-fashioned ones, and the effort with more modern machines would have been far lower.

Figure 10 is an example of a relatively light job. The man was working with an autogenous welding machine, welding the sheets of the car-body. He had a caloric output of 1 kcal/min, a very low pulse rate and rather long waiting-times. In Fig. 11 very similar work is done but an electric spot-welding system is used. The welders, which are rather heavy, are counterbalanced. This man has even more waiting-time than the man before and about more than 5 per cent voluntary rest pauses. In addition, he has enough time to help his neighbour. The caloric output is a little higher but the pulse rate is far higher than

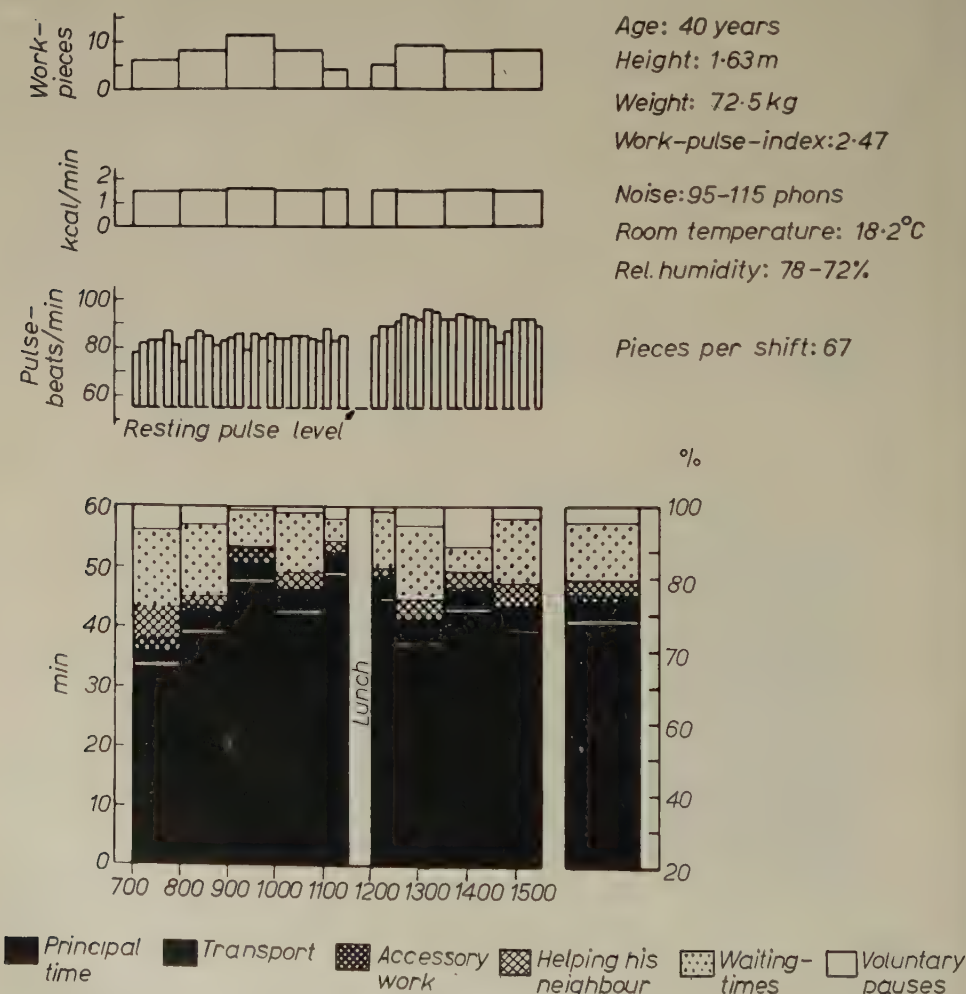

Figure 11. Spot-welding of car body.

that of the previous man and shows a tendency to increase. This is probably the consequence of muscular fatigue caused by static work. Although the welding machine is counterbalanced, it requires a lot of static work to draw the tong-shaped system to the point where the sheets are to be welded. That appears to be the cause of the rising pulse rate. A comparison of Figs. 10 and 11 shows that the use of more modern equipment—in this case the point welder—does not necessarily mean less effort for the worker.

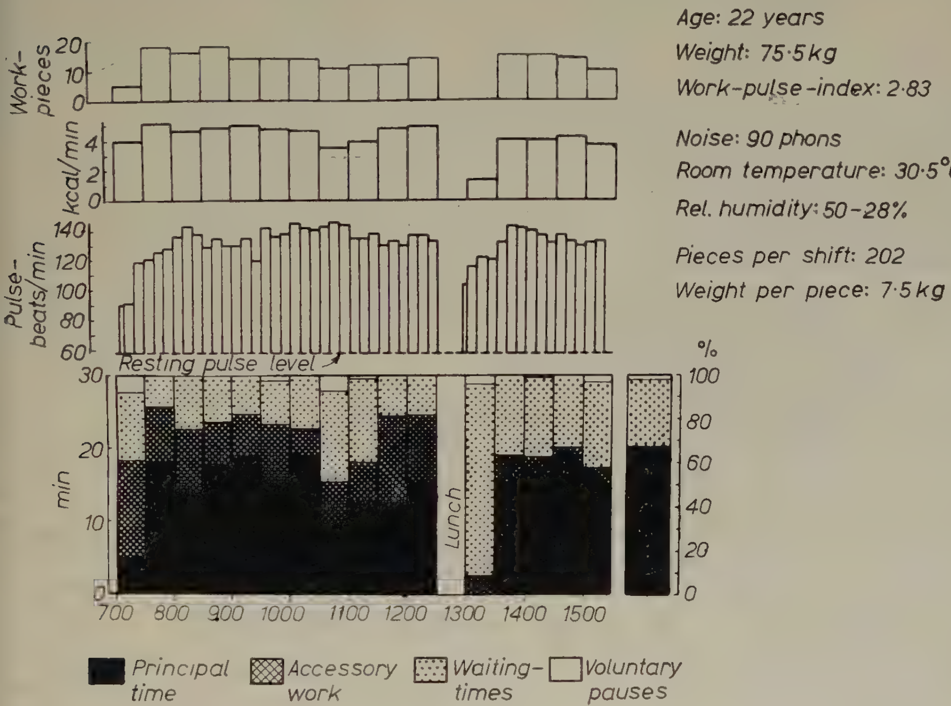

Figure 12. Forging gear wheels.

Figure 12 was taken from a man making gear wheels. His work-place was in a rather poor condition. On the average, he was forging only 45 per cent of the shift. Accessory work was more than 20 per cent, and we are sure that most of his waiting-times were voluntary, but camouflaged, rest pauses. Besides this, transport work amounted to more than 30 per cent. But the average caloric output was higher than 4 kcal/min. This means that already, from the energetic point of view, the man was overloaded. The decreasing number of work-pieces per minute is here evidently a consequence of rising fatigue, and——what was most striking—his pulse rate reached 140, even 150, heat stress being very large. The temperature of the room was 30·5°C and the exposure to radiant heat during work was very great. It was possible to improve his work-place, to reduce the caloric output by more practical arrangement of the work and to decrease heat stress by better location of the furnace and by protecting shields.

Figure 13 shows a survey of the 18 men we had under examination in this plant. The resting-times—i.e. waiting-time plus voluntary rest pauses—vary

from 8 to 40 per cent and, what seems more impressive, accessory work takes from about 5 to more than 20 per cent of the whole shift. As a general rule, the time for accessory work is longer when the rest pauses are longer also. This proves that a large part of the accessory work is camouflaged rest pause. Sometimes such rest pauses may be necessary to give the man sufficient recovery time. It always seems to be the tendency of the workers to make as few visible rest pauses as possible. They do not like to appear idle. Even with very heavy work, when, from the caloric output, we can calculate rather exactly the rest

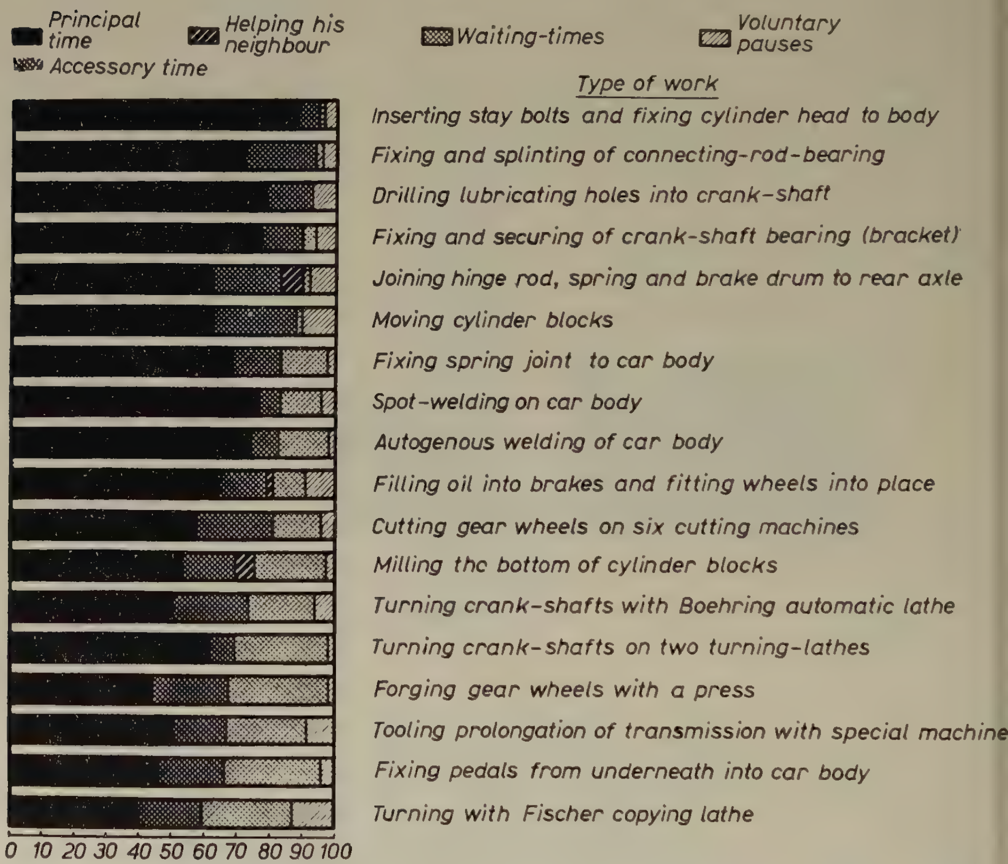

Figure 13. Distribution of working- and resting-time during an 8 hour-shift, taken from 18 work-places in a motor-car factory.

pauses needed, we always find that the actual rest pauses are shorter than the calculated ones. Instead of really pausing the workers prefer to slow down the speed of work or to do accessory work in order to appear occupied. From a physiological point of view, this is certainly not the right way because a real rest pause gives much more recovery than a pause camouflaged by some light work.

One of the causes of this situation appears to be the method of supervision of the workers, especially by the junior supervisors. Their principle is that

every man must be occupied all the time, and a man doing nothing for a certain time is regarded as lazy. Therefore, it is much better for rest pauses to be organized by management than for the organization to be left to the discretion of individual workers. In an example like that quoted, it is not possible to organize very short pauses because the work cycle is about 7 to 8 minutes and cannot be interrupted. We suggested to this factory that the workers should have an additional rest pause of 15 minutes about three hours after the beginning of the shift. We are sure that these rest pauses neither reduce the actual working-time nor the output, but reduce the voluntary pauses, the so-called waiting-times and the times for accessory work.

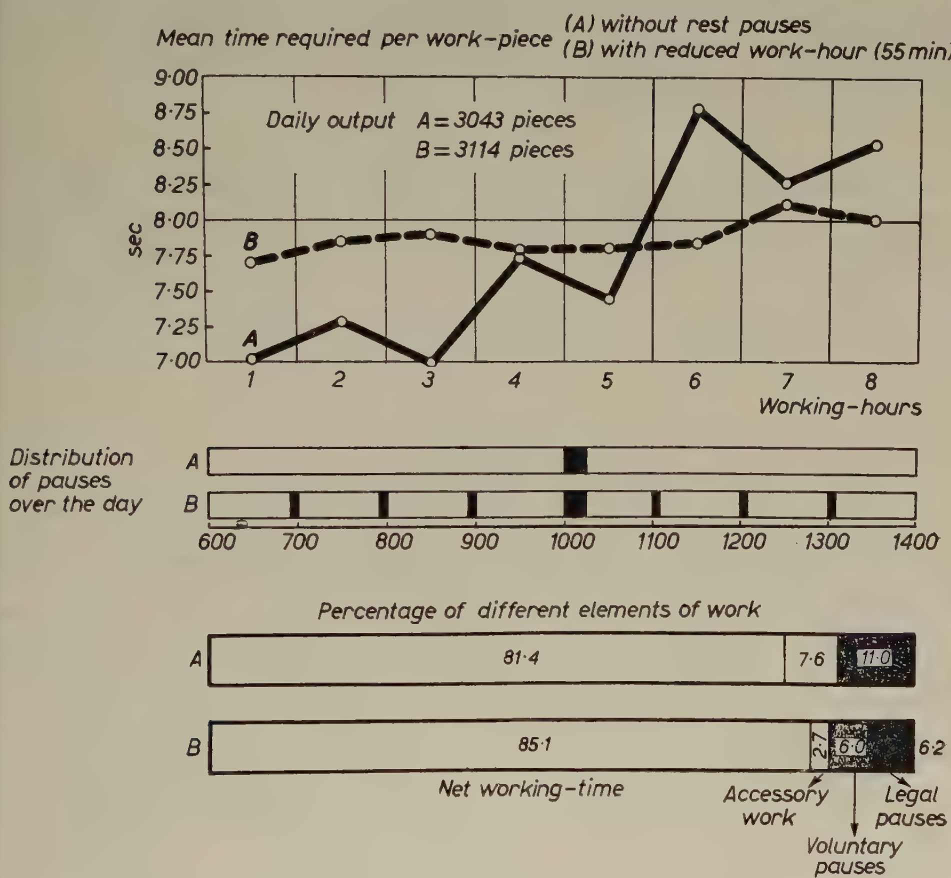

Figure 14. The influence of short pauses on output and net working-time.

It was not possible to demonstrate the result of making this change in work organization, but this was done in another factory (Fig. 14). The female worker concerned was making very small fuses for radio receivers. The increasing time per work-piece shows the increasing fatigue over the day. This girl had to work without organized rest pauses except the legal pause at noon. At first glance, she seemed to take no voluntary pauses at all, but time-studies showed that she had very short voluntary pauses occupying about 11 per cent of the whole shift, and, furthermore, she used 7·6 per cent of her working-time for accessory work. When rest pauses of 5 minutes per hour were introduced, the

output per day rose from 3043 to 3114 pieces. The voluntary pauses were reduced from 11 per cent to 6 per cent and the time for accessory work from 7·6 per cent to 2·7 per cent. This is another proof that a great deal of accessory work means camouflaged rest pauses. It is far better to organize rest pauses, and, thereby, avoid fatigue than to let the man make voluntary or camouflaged pauses as a consequence of fatigue.

La mesure de la charge de travail dans l'industrie nécessite l'emploi des méthodes auxquelles on peut se fier et des appareils à la fois solides et indéréglables. Les trois méthodes suivantes sont employées habituellement dans l'Institut de l'auteur: 1° chronométrage exact pendant la journée entière, comprenant les détails de toutes les opérations auxiliaires et de tous les repos, 2° mesure de la dépense d'énergie pendant le travail au moyen d'un respiromètre construit dans l'Institut, et 3° mesure de la fréquence du pouls au moyen du compte-pouls de Müller.

On cite quelques exemples de l'emploi de ces méthodes dans quelques ateliers de forge et une usine d'automobiles. Les exemples comprennent les cas où l'ouvrier était chargé insuffisamment ou excessivement, et l'on en discute les causes et remèdes.

Messungen zur Bestimmung der Arbeitsschwere, wie sie in der Industrie vorkommen, erforden die Verwendung zuverlässiger Methoden, sowie von Apparaten, die stabil gebaut und betriebssicher sind. Drei Methoden sind für diese Messungen in unserem Institut besonders gebräuchlich : 1. Eine genaue Arbeitsablaufstudie während des ganzen Arbeitstages einschliesslich aller zusätzlichen Nebentätigkeiten und aller Ruheperioden ; 2. Messung des Energieverbrauchs während der Arbeit mit Hilfe des Respirometers, das in unserem Institut entwickelt wurde; 3. Messung des Pulsfrequenz mittels des Pulszählers nach E. A. Müller.

In mehreren Beispielen aus Schmiedewerkstätten und einer Kraftwagenfabrik sind diese Verfahren zur Anwendung gekommen. Die Beispiele erfassen Fälle mit zu geringer wie mit zu grosser Belastung des Arbeiters. Es werden die Gründe und ihre eventuelle abhilfe diskutiert.

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PRODUCTION WELDING IN EXTREME HEAT

By K. A. Lifson

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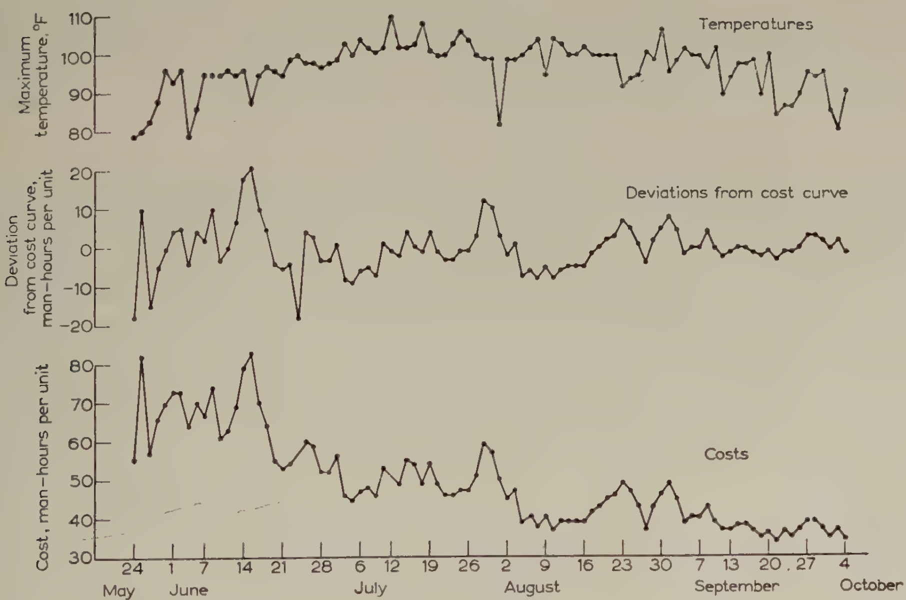
Daily productivity figures for a group of welders were compared with daily temperature maxima during an unusually hot summer. No correlation was found.

The ill wind which brought Dallas the fiercest summer in its history also blew in what looked like an opportunity to measure decrement in productivity of heavy labour performed in extreme heat.

In April of 1954 labour standards were very accurately determined for welding about thirty sub-assemblies of a large unit. Each sub-assembly was assigned a value as a percentage of the whole large unit, the percentage value being based on the ratio of the standard man-hours on the sub-assembly to the standard man-hours on the large unit. An exact count of the number of each sub-assembly accepted by inspection was kept daily, and by totalling the quantities multiplied by the respective percentages, the 'equivalent units' completed in a day were calculated. The man-hours charged to welding this unit each day, divided by the equivalent units produced, gave an accurate man-hour per unit cost figure.



Temperatures and costs per unit.

The situation seemed to be one ideally suited to investigate changes in productivity related to changes in the environment. Enough welders—thirty to forty—were working on the unit so that casual variation in individual productivity from day to day would average out rather well. The shop layout, equipment, methods of working, job assignments, raw material condition, and

acceptable quality level were standardized by the middle of May. Enough units were to be built so that the work would last about nine months. A sensitive and accurate measure of productivity existed. Everything was ready for the study except the change in external environment—and then the Dallas summer provided that in the form of many very hot days, some of them the hottest ever recorded for Dallas.

Welding is an uncomfortable job in any degree of heat. Heavy gloves are worn, as is a rather heavy face mask, supported on the brow by a 'sweat-band' which really deserves the name. The welding machines create a lot of heat themselves.

The working conditions being discussed were hotter than usual. The work was done under a tin roof with no insulation. Also, the work itself involved lifting, carrying, and turning the sub-assemblies which averaged about 75 lb each. All of these conditions would point to a strong correlation between productivity and temperature.

The revealed absence of any correlation seemed even more dramatic than the expected correlation would have been.

The figure is a plot, by working day, of Dallas weather bureau temperature maxima, man-hour cost per unit, and deviations of cost from the improvement curve of best fit. The coefficient of correlation between temperatures and deviations from the improvement curve was an insignificant $-0{\cdot}08$.

The data, including those plotted in the figure, and daily mean temperatures and relative humidities and quantities produced, will be available on request to anyone who can discover anything else with them.

In closing, a comment of a welder is worth passing along : "The temperature doesn't mean as much as the heat you get from the office".

Les chiffres de productivité journalière pour un groupe des soudeurs ont été comparés avec les températures diurnes maxima pendant un été exceptionnellement chaud. On n'a pas trouvé de corrélation.

Die Tagesleistungszahlen einer Schweissergruppe wurden mit den Tagestemperaturmaxima während eines äusserst heissen Sommers zusammengestellt. Es wurde keine Korrelation gefunden.

A STUDY OF THE INFLUENCE OF VIBRATION ON MAN

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The effects of vertical and horizontal mechanical vibrations up to 100 cycles per second on the human being were examined by physical and physiological methods. Resonance phenomena are described. A strain scale is given for vertical and horizontal vibration excitation. Special examinations of the movement of the head show elliptic vibrations in spite of linear excitation. Vibration measurements in a rail-motorcar provide an example for typical resonance phenomena of the mechanical system formed by a 'man sitting on a seat'.

§ 1. Introduction

The influence of mechanical vibrations on the human body was studied in the standing and sitting positions. The range of frequencies investigated, up to 100 cycles per second, is significant for industry and transport. The response of the human body as a mechanical system was examined, and measurements of the effects of vibration were made by means of objective physiological tests and by subjective assessments. In this way it was possible to determine the degree of strain experienced by the subjects. Two vibration generators were used, one for frequencies up to 8 cycles per second, the other for frequencies above 5 cycles per second. Vertical and horizontal oscillations were applied to the platform on which the subjects stood or sat.

§ 2. Methods and Results

By means of acceleration meters the amplitudes of movement of various points of the body were compared with the amplitude of the vibration platform at different frequencies. Figure 1 shows typical results with vertical vibrations. The absolute amplitudes applied to the subjects were smaller at higher frequencies than at lower frequencies, but in Fig. 1 these various amplitudes are represented by a scale value of 100 per cent at all frequencies. In the upper part of this figure the relative amplitudes of the shoulder and the head are expressed in 'per cent' of the platform amplitude. At the lowest frequencies the movement of the body follows that of the platform. Near 4 or 5 cycles per second there is a resonance maximum of body movement. Higher frequencies induce lower amplitudes of body vibrations. If the vibration of the shoulder is taken as 100 per cent and compared with the movement of the head, the maximum relative amplitude of head movement is found to occur between 20 and 25 cycles per second, as shown in the lower half of Fig. 1.

A further method of physical investigation is the measurement of the force transferred to the human body by the vibration platform. This force depends on the frequency and amplitude of the exciting system and also on the mechanical properties of the vibrated system, the human body. In a mechanical system, the force divided by the velocity yields a quantity termed the 'mechanical impedance'. Figure 2 shows this quantity, the

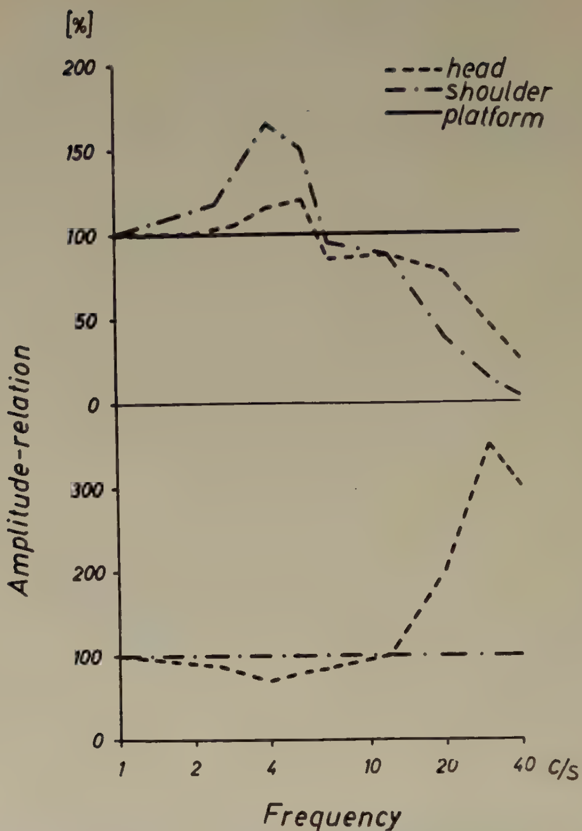

Figure 1. Relation of amplitudes of vibration : upper part, head and shoulder amplitude in relation to platform. Lower part, head amplitude in relation to shoulder amplitude.

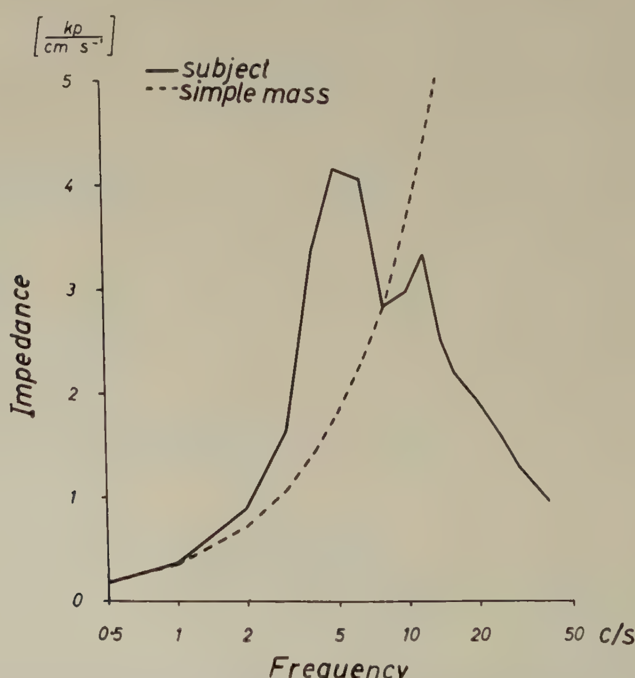

Figure 2. Mechanical impedance of a standing subject : full line, measured on subject ; broken line, computed for simple mass.

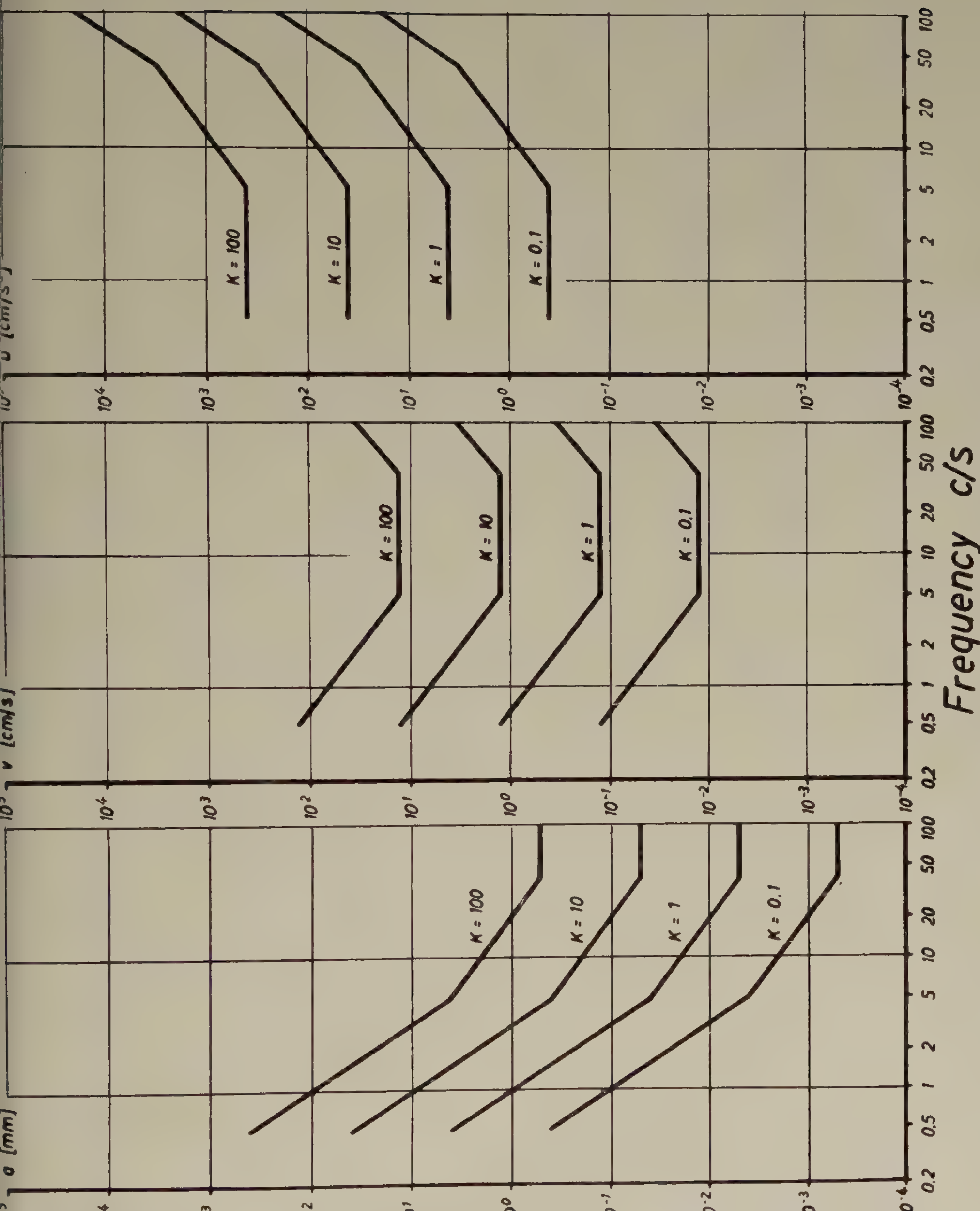

Figure 3. Scale of strain for vertical vibrations.

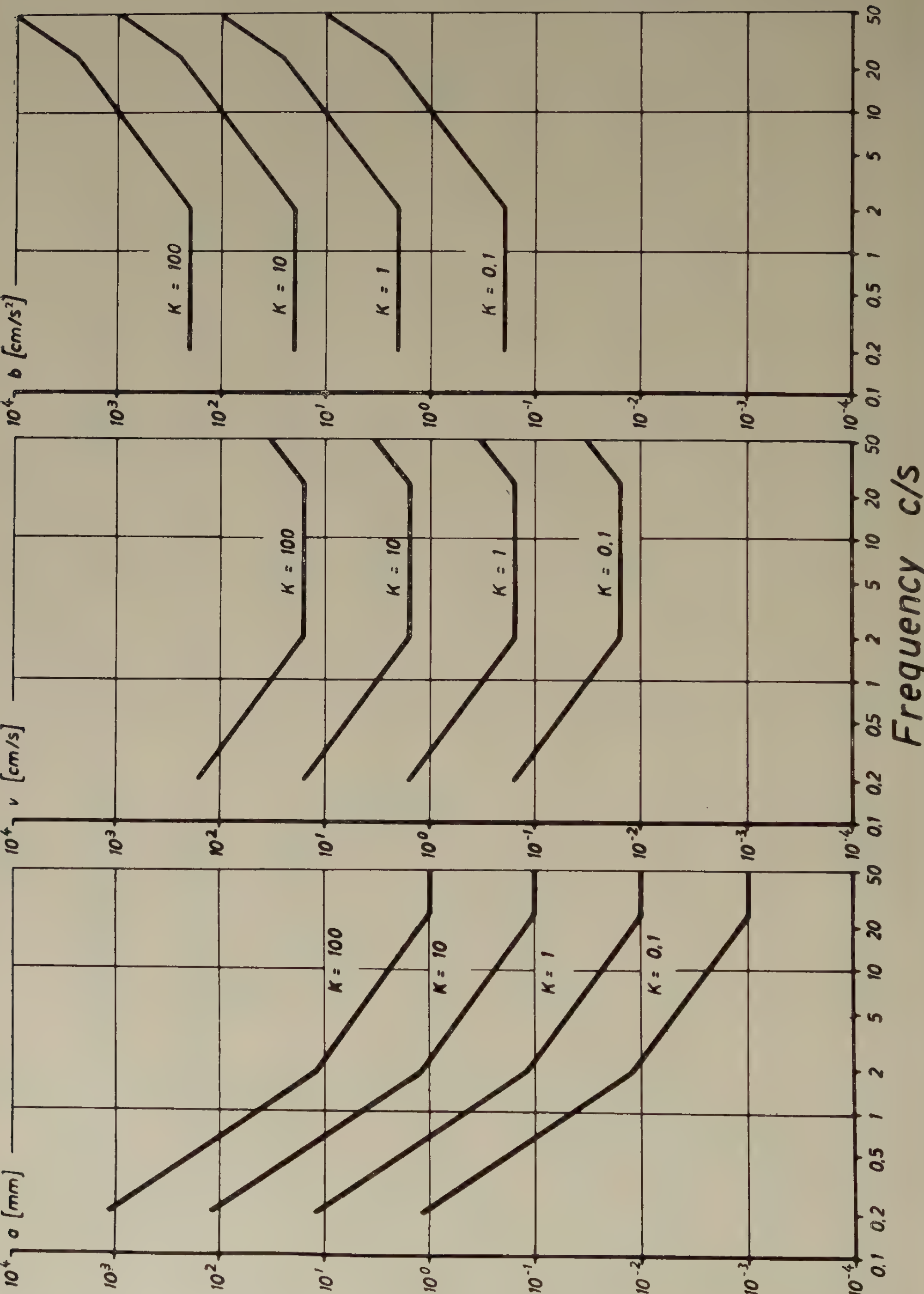

Figure 4. Scale of strain for horizontal vibrations.

transferred force divided by the velocity of the vibration platform, measured at different frequencies ; the subject was standing. This graph can be called the ' mechanical impedance ' of the body, measured in ' number of kiloponds per unit of vibration velocity ', i.e. kp/cm s^{-1}. The characteristics of this diagram are the same for any other standing person. The maximum of

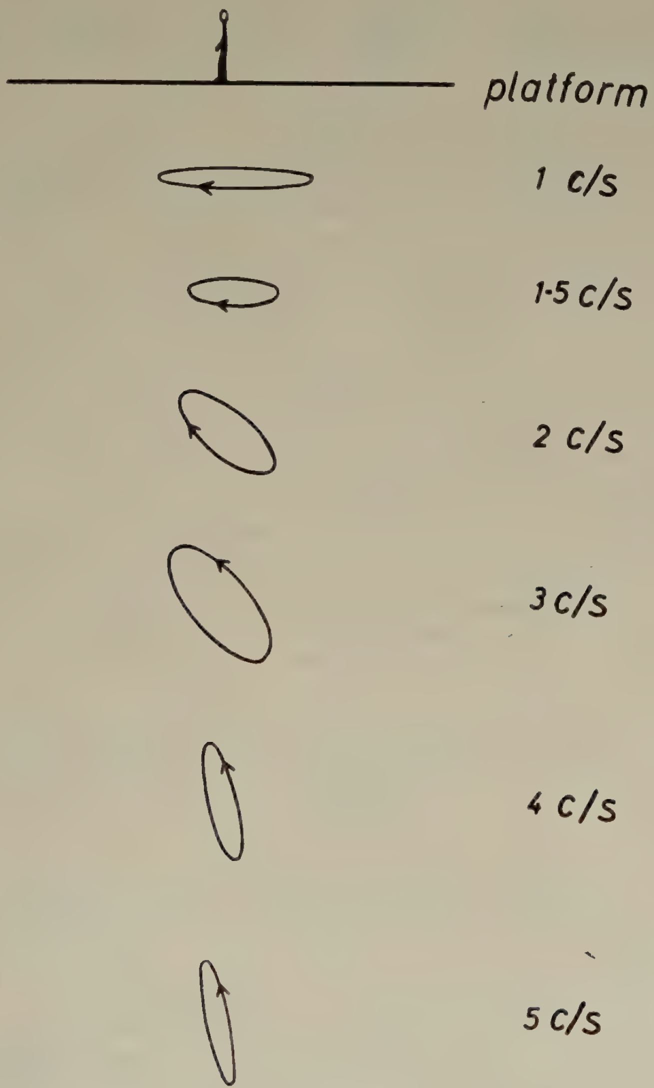

Figure 5. Head-movement of a standing subject for horizontal vibrations of platform.

resonance again lies near 5 cycles per second, a further but lower maximum is to be observed at 12 cycles per second. The calculation of the ' impedance ' with the human body considered as a simple mass of the same weight results in the curve shown by the broken line. In this case the force is proportional to vibration acceleration. The shape of the impedance curve of the human body shows that in relation to impressed vibrations the human body cannot be

looked upon as a simple mass. Phenomena of resonance prove the human body to be a damped system of masses and springs. These resonance-phenomena acting on the human body, in particular the natural frequency of some 5 cycles per second, are quite uncomfortable.

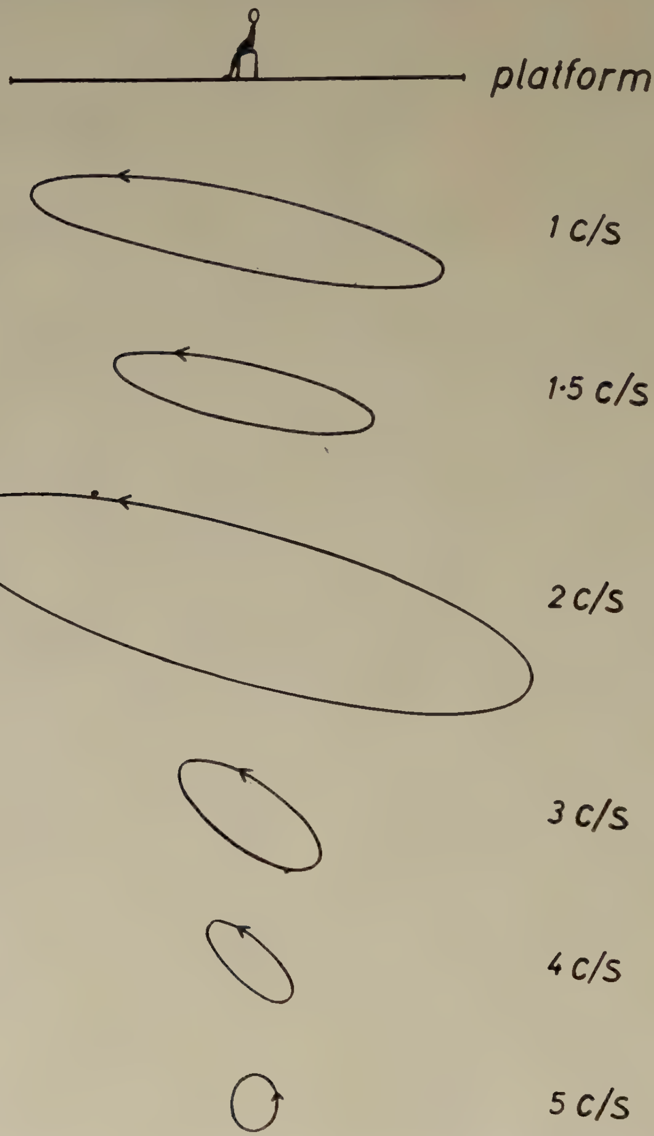

Figure 6. Head-movement of a sitting subject for horizontal vibrations of platform.

§ 3. Scale of Strain

Physiological experiments, for example, the alteration of the electrical resistance or capacitance of the skin as a 'vegetative' reaction under the influence of vibration, in combination with the subjective assessments, were used to determine the strain on the human body under the influence of vibration. By these methods of investigation, a scale of strain was developed.

Figure 3 shows strain for vertical vibrations and Fig. 4 for horizontal vibrations. The symbol 'K' is used to represent the degree of strain. Up to the present mainly logarithmic scales have been used for physiological and psychological quantities occurring in such problems, but they often prove inadequate. For this reason the K-scale is non-logarithmic. Figures 3 and 4 show the K-scale for discrete values in the range of $K=0{\cdot}1$ to $K=100$. On the left of each figure is shown the amplitude (a) of vibration plotted against frequency, in the middle the velocity (v) and on the right the acceleration (b). If subjects are vibrated vertically with constant vibration velocity at all frequencies, i.e. velocity $=2\pi \,.\, f \,.\, a=$ const. (amplitude $=a$, frequency $=f$), the strain on the human body is largest in the range of frequencies from 5 to 40 cycles per second. In this range strain is satisfactorily measured by the velocity. Below 5 cycles per second the acceleration of vibration $(=4\pi^2 \,.\, f^2 \,.\, a)$, and

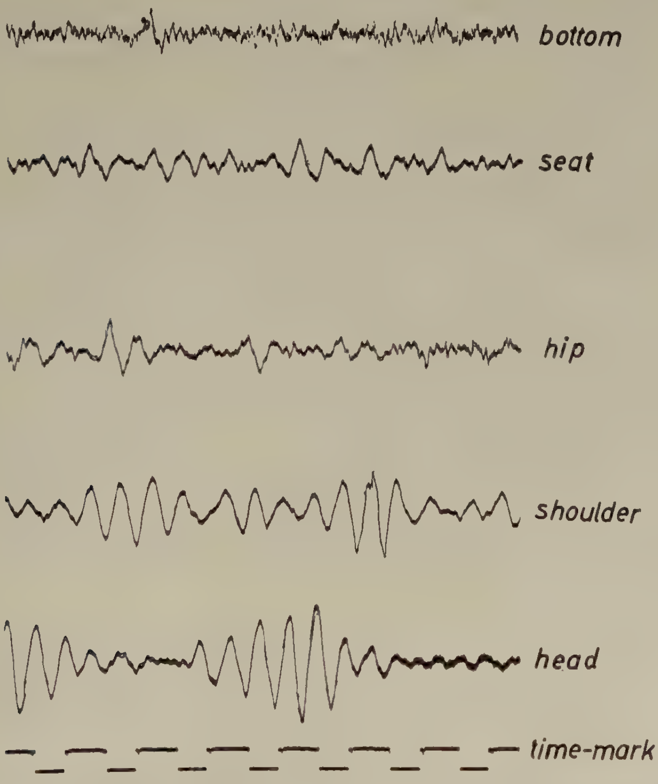

Figure 7. Vertical vibrations of seat and man in a rail-motorcar. The top two records show car vibration, the lower three show vibration amplitudes of the subject. Time marker, 1 sec.

above 40 cycles the amplitude $(=a)$ serve as standards for measuring strain. For horizontal vibrations the equivalent ranges of frequencies are : below 2 cycles per second acceleration ; from 2 to 25 cycles per second velocity ; and above 25 cycles per second, amplitude.

The significance of the K-scale, founded on experimental and practical experience, was reached in cooperation with several vibration experts. A K-value of 0·1 represents the lower limit of perception, a K of 1 may be

allowed in industry for any period of time, a K of 10 should be allowed only for a short time. K of 100 is the upper limit of strain for the average man.

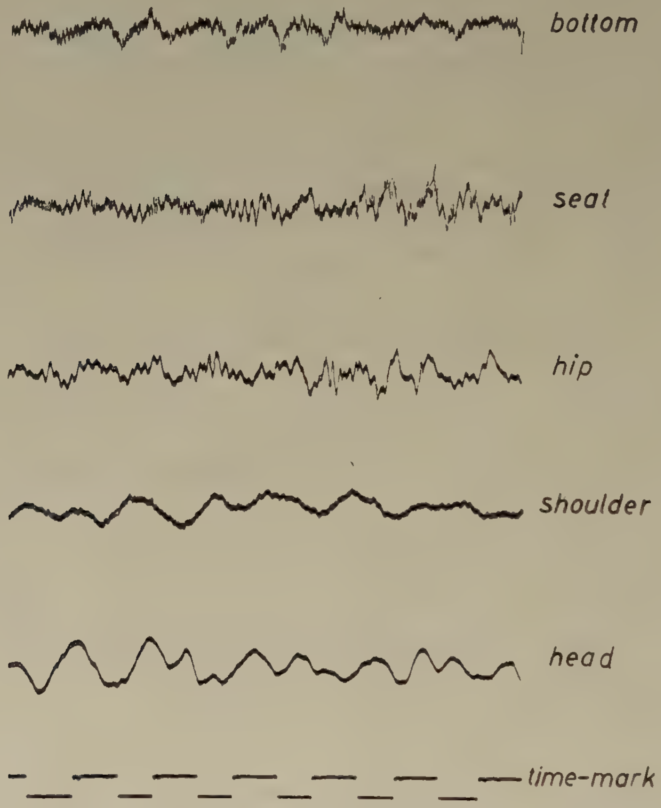

Figure 8. Horizontal vibrations of seat and man in a rail-motorcar. The top two records show car vibration, the lower three show vibration amplitudes of the subject. Time marker, 1 sec.

§ 4. Head-Movement with Horizontal Vibrations

The movements of the head of a subject standing on a horizontally vibrating platform are elliptical as shown in Fig. 5. With increase of frequency the form of the ellipse becomes more vertical, so that at 4 or 5 cycles per second the movements are mainly vertical although the platform oscillates horizontally In Fig. 6 the head-movements of a sitting person under horizontal vibrations are to be seen. The large elliptical form at 2 cycles per second results from resonance-phenomena. Possibly the elliptical form of head-movement may be a factor inducing motion-sickness.

§ 5. A Practical Example

The oscillograms in Figs. 7 and 8 are vibrations in a rail-motorcar, measured on seat and man. The higher frequencies are damped by the seat, while the amplitudes of the lower frequencies, of about one to two cycles per second, increases. The reason for this is that the mechanical system of seat and man

oscillates in resonance. A better system of damping and springing in the seat, in relation to the elastic properties of the human body, would avoid these effects, which are uncomfortable and may even be dangerous.

L'influence que des vibrations mécaniques, aussi bien verticales qu'horizontales, jusqu' à concurrence de 100 Hz, exercent sur l'organisme humain, a été examinée à l'aide de méthodes physiques et physiologiques. Les phénomènes de la résonance sont décrits. Une échelle de la tension due à l'excitation causée par des vibrations verticales et horizontales est représentée. Malgré une excitation linéaire, des examens particuliers des mouvements de la tête montrent des vibrations elliptiques. Des mesurages des vibrations d'une automotrice fournissent un exemple de la résonance typique du système mécanique ' siège-homme '.

Der Einfluss vertikaler und horizontaler mechanischer Schwingungen bis 100 Hz auf den Menschen wurde mit physikalischen und physiologischen Methoden untersucht. Resonanzerscheinungen werden geschildert. Ein Belastungsmassstab für vertikale und horizontale Schwingungserregung wird dargestellt. Spezielle Untersuchungen der Kopfbewegungen zeigen trotz linearer Erregung elliptische Schwingungsformen. Schwingungsmessungen in einem schienengebundenen Triebwagen geben ein Beispiel für typische Resonanzerscheinungen des mechanischen Systems ' Sitz -Mensch '.

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FAULT-FINDING IN ELECTRONIC EQUIPMENT

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The problem of training men to locate faults quickly and correctly has only recently been recognized. In the past it was thought that the acquisition of knowledge of electronic theory was a sufficient preparation for the job. But observation shows that in general men trained in this way do not search for faults in the best possible ways ; they frequently make serious errors of strategy. The use of laboratory tasks, in which the secondary difficulties associated with the real job through poor Human Engineering, and a lack of suitable data books and diagrams are eliminated, shows that most men do not search efficiently. Their behaviour is influenced to a considerable extent by irrelevant factors ; it changes if tasks are presented in different ways ; and in a given task irrelevant features of the display affect their procedure.

The appropriateness of a man's strategy appears to be positively correlated with his intelligence. This suggests that if the intelligence level of men recruited for fault-finding training is reduced, then not only will they experience more difficulty in assimilating electronic theory but when they come to the actual job they will use less effective strategies in their fault-finding. The problem of training them to use better strategies is discussed.

Experiments by the author are described and set in relation to previous work.

§ 1. Introduction

The key to the effective maintenance of most present day electronic equipment lies in the rapidity with which faults are located once they have occurred. This is also true to some extent of mechanical, pneumatic, hydraulic and other kinds of equipment. Although it is possible to design self-monitoring devices the task of fault-finding is likely to be of importance for some time to come, if not for always. Self-monitoring devices are expensive and, in any case, the fault-finder is needed whenever the monitor itself becomes faulty.

Observations of fault-finders at work on electronic equipment made by Crowder (1954), Dale (1958 a), and Saltz and Moore (1953) have revealed a low standard of proficiency. Crowder, whose study is probably the most adequate, comments the most strongly : " The mechanics appeared to work to a great extent by trial and error . . . they made a sufficient confirmatory check before replacing a subunit in only 203 of 600 problems where such a check was possible and necessary " (see p. 28). If, by coming to understand the behaviour of fault-finders, we can reduce this lack of proficiency, research will be well worthwhile. Attempts to gain an understanding of this behaviour are reported in this paper, but before they are described a brief account of the task of fault-finding is given.

The writer has drawn to a considerable extent upon the review by Standlee *et al.* (1956) in which synopses of 108 papers are given. It is recommended as a source.

§ 2. The Nature of the Task

Rulon and Schweiker (1956) have pointed out that fault-finding, or trouble-shooting to give it its American name, has long been considered an art. A

U.S. Navy training manual (Anon 1952) confirms this view: " Trouble-shooting ability is a sort of ' sixth sense ' that you develop with experience " (p. 192). No single recognized way of setting about the task exists. Indeed, the fact that it is regarded by some workers as problem solving (Gagné 1954, Ray 1955) is implicit recognition that the individual finds his own way and does not directly apply previously learned rules. But this is not always the case. Bryant *et al.* (1956) prefer a broad definition of the task to include " . . . any activity which is directed expressly to the correction of certain classes of malfunctions. A problem exists when a goal is recognized but the meaning or route to the goal is not immediately clear . . . all trouble-shooting is not problem solving, since for many trouble shooters the path to the goal is quite clear-cut and routinized. Nevertheless a considerable overlap exists . . . in so far as a trouble shooting attempt contains exploration of the situation to determine the crucial elements, alternative approaches, and the relationships between them, then it involves the type of behaviour usually called problem solving " (p. 121). The task encountered by a man then will depend upon the way he sets out to do the job rather than upon the job itself. Miller (1953) has, on these lines, offered two descriptions and the writer has said elsewhere (Dale 1958 d) that the task can be reduced to a simple routine (although this course is not to be recommended).

Although we cannot define the task strictly it is possible to describe the demands of the job, at least in broad terms. It is essentially a form of searching. An equipment will usually contain many parts, perhaps hundreds or thousands, and a failure which leads to its breakdown is commonly caused by a failure of just one of these. The job is to locate it as rapidly as possible. At this point the most efficient ways of searching will be described but these are ideal in a logical rather than a psychological sense. (This point will be brought up again later.)

It is best in a search of this kind to begin by asking general questions if this is possible. Questions about particular parts of an equipment will on the whole yield little information early in the search, for the same reason that it is unwise to ask in the game of 20 Questions, knowing only that the ' thing ' is mineral, " Is it the brass door knocker at No. 10 Downing Street ? " This point has been made by Miller *et al.* (1953 a) and by Broadbent (1955). Whether or not it is possible to ask general questions at all depends upon the nature of the system to which the parts belong and the nature of the tests which it is possible to make. If the parts can only be examined one by one, either by virtue of their independence or the nature of the available test-gear, then clearly no general questions can be asked. But where they belong to groups and the overall performance of these groups can be measured then it is indeed possible to ask general questions.

The overall function of an electronic equipment is to transform a signal in some way. A simple commercial radio set transforms a signal in the form of weak electromagnetic waves into the form of powerful electrical waves which will drive a loudspeaker. This is not done in one step. Within the radio there are a number of functional units called stages. Each of these will by itself effect only a small change in the form of the signal. The overall result is achieved through a succession of small changes and the structure of the simple radio reflects this ; it consists basically of a single

chain of stages. In more complex equipments the flow system will be more involved ; there may be several signals ; some generated within the equipment, which may be mixed together and separated again at another point ; there may even be loops, the signal being tapped off at one point, modified and then fed back into an earlier part of the chain. But in all these cases stages are used as functional units which modify the signal sequentially.

In isolating a faulty stage the fault-finder can test chains of stages by checking that they function correctly as a whole. If when an appropriate signal is fed into one end of a chain it appears correctly transformed at the other end, then all the parts within the chain must be functioning correctly. It is by making this kind of functional test that the fault-finder can ask general questions.

When the faulty stage has been found it is necessary to search within this stage for the part which has failed. Although the parts within a stage are functionally related they cannot be examined in groups in the way that the stage can be tested, and the location of a faulty part is generally a matter of testing each part and each connection in turn. Searching for a faulty stage can be described as searching in a structured system, for it is by using knowledge of the structure of the system that a faulty stage can be most rapidly located. Searching for a faulty part within a stage can be described by contradistinction as searching in an unstructured system.

The best way of searching for a fault in a structured system containing units (or stages) which are equally likely to be at fault, is to ask questions which each reduce the number of possibilities by a half (see Miller *et al.* 1953 a). If some units are more likely to be faulty than others the question should be such that it divides the possibilities into two equi-probable groups and determines which of these is faulty. But probability is not the only extra factor which should ideally be taken into account. Some tests may be made more easily than others and, if the time taken to locate a fault is to be used as a criterion of efficiency, it might be better to make a rapid test which divides the probabilities unequally rather than to make a lengthy test which is otherwise ideal. Thus effort is a third variable. Hoehn and Saltz (1956) have discussed this matter in more detail, as have Stolurow *et al.* (1955).

From this it can be seen that the choice of an ideal test is a complex matter. The main point, however, is that the search should proceed from the general to the particular. Gross divergencies from this rule, such as those quoted above from Crowder, lead to considerable waste of effort, but a failure to choose ideal tests on the basis of structure, probability, and effort, is not generally serious. When the units are treated separately, as are the parts within a stage, the basic requirement is that the search should be systematic. If the units are equally likely to be at fault, then any one order is as good as any other ; the only possible cause of inefficiency is that the results of some tests might be forgotten and then have to be repeated. If some parts are more likely to be faulty than the rest or are easier to test, then it is clearly best to examine these first.

The presence of a fault in an equipment is indicated by some change in its overall function. By careful examination of this change it is sometimes possible to determine which region is faulty. This is especially clear when the equipment has multiple functions such as the domestic television receiver

which reproduces both sound and vision. If the sound is faulty while the vision is normal then the fault must be in the chain which leads to the loud-speaker rather than that which leads to the tube. Diagnosis from the change in overall characteristics in this way can proceed just as the process already described. It is commonly the best first step in fault location since the effort involved in checking performance is usually small and because it provides a means of examining the most general questions. Sometimes the fault can be localized to a very considerable extent by using very fully the information present in the symptoms, so that in some cases it is possible to go straight from symptom to the faulty stage or even the faulty part. To sum up: logically efficient fault-finding proceeds through three phases, an initial deduction based upon the change in an equipment's overall functional characteristics ; a search for the faulty stage, in which use is made of the signal flow path ; and finally a search within the faulty stage for the faulty part.

Instead of searching in the way described above it is possible to attempt the job of fault-finding by learning symptom-cause associations by rote. This approach to the job has formed the basis of research by Stolurow *et al.* (1955) into determining the best way to teach these associations. When an equipment is of any size, however, it is clearly pointless to attempt seriously to learn all possible symptom-fault associations.

This criticism, based as it is upon psychological grounds, brings us back to the fact that the logically ideal methods of fault-finding may differ from what are psychologically ideal. This is an empirical matter which has yet to be investigated. The task of fault-finding, i.e. the best method from the engineer's point of view, cannot, therefore, be defined, but one thing is clear and that is that it is some form of searching.

2.1. *Sources of Difficulty*

The choice of the best strategy of searching presents the essential difficulty of the task. But apart from this it is found that there are also secondary difficulties. It has been assumed so far that all-or-none tests (which indicate presence or absence of a correct signal) can be made to check signal flow in an equipment, but this is not always the case. Sometimes, when the effect of a fault is to distort the signal, it is possible to measure a number of parameters of the signal, some of which give more sensitive indications of its state than others. At present the onus is upon the engineer to learn by rote the best testing techniques. Handbooks are prepared for equipments but the data is commonly in such a form that it cannot be found quickly unless the handbook has previously been studied extensively. In a complex equipment, where it is virtually impossible for the engineer to learn all the best techniques by rote this is a serious matter. Lack of adequate data manuals were given specific mention by both Saltz and Moore (1953) and Dale (1958 a). Another difficulty is that diagrams often are not as clear as they might be ; they sometimes even fail to show test points (see Dale 1958 a). (Rulon and Schweiker (1956) have also noted the need for adequate diagrams.) Test-gear is often poorly designed. Bryan *et al.* (1956, p. 171) noted in some field observations : " Almost every performance contains minor errors of test

instrument usage ". Since this was found in the activity of experienced men it is a reflection upon the test-gear rather than those using it.

These secondary difficulties are very real and important ; they certainly deserve urgent attention but they will not be further discussed in the present paper which is concerned rather with the essential difficulties of fault-finding. Recommendations for reducing these secondary difficulties are to be found in Spector *et al.* (1955), Miller and Folley (1951), Miller *et al.* (1954) and Shackel (1957). The development of special diagrams (diagnostigrams) is reported by Ellis (1958).

§ 3. Basic Questions and Method

In order to discover how to train efficient fault-finders answers must be found to three fundamental questions. (1) What should the men do ; (2) What do they do without any special training ; (3) How best can they be trained ?

As the above discussion has shown, no definite answer to the first question can yet be given. The men would be most effective if they were to use the logically ideal methods of searching. There is no reason to suppose that these methods impose any great strain upon the searcher's abilities and in the absence of evidence it seems reasonable to accept the method described above as being the best way of doing the job. It was pointed out then that where differences of probability and effort complicate the issue it would be difficult to compute the ideal choice of a checkpoint. But efficiency is not noticeably impaired if the choice is slightly imperfect, so perfection does not matter a great deal. Provisionally it can be said that men should approximate to what is logically the ideal strategy.

An alternative answer to this first question has been offered elsewhere ; this is that all men should do what the best of them do already. The study by Warren *et al.* (1955) and the subsequent proposals made in it, are based upon this approach. There are serious disadvantages to this way of tackling the problem. For one thing, there is no guarantee that the best men, in the sense of the most successful or useful men in the eyes of their superiors, have found the best methods. They might do better still with other, untried, techniques. The method advocated by Warren *et al.*, although sufficient, is certainly not very efficient. Then there is the other possibility that these men have evolved methods which best suit themselves but which would not suit others.

The experiments to be reported are concerned with the second fundamental question : ' How do persons search without special training ? ' Laboratory tasks which are analogous to the searching tasks encountered in fault-finding, have been devised in order to study the strategies subjects adopt under different conditions. This technique can be used to study the behaviour of trained fault-finders working in controlled conditions and to relate their performance to their ability on the job but this has not yet been done.

Compared with on-the-job studies this method has certain advantages :

(1) The laboratory task is free of what have been referred to as secondary difficulties.

(2) The factors which govern the ideal choice of strategy can be readily controlled ; the relative probabilities of fault locations can be stated, the relative effort required to make different tests can be indicated and the structure of the simulated equipment can be very clearly displayed. This is not the case if we study men at work on real equipment.

(3) The experience subjects encounter with the tasks can be readily controlled. They will not have met the task before they arrive in the laboratory and their orientation to it can be manipulated by varying instructions. By using the apparatus to be described below the success they achieve when they use a given strategy can also be controlled.

(4) A much wider range of subjects can be used, since specialized knowledge is not required to perform the laboratory tasks. This means that broad questions can be examined. Trained engineers have been selected (if only by attrition). If their strategies are examined the results might only apply to a small subsection of the population. By using a simple task it is possible to examine a larger section of the population and to answer questions about the strategies that can be expected of the population as a whole.

(5) By using subjects who have not had specialized training a clearer picture can be gained of the way persons search. Observations of trained engineers might depend to a considerable extent upon their training. Furthermore, if we wish to examine the factors which are important in training, it is better to start with untrained subjects. Relearning differs in some ways from initial learning and the results obtained from relearning experiments might be misleading if applied in an initial learning situation.

(6) It is practically more convenient to work with a laboratory task. The equipment involved is far less complex and requires less attention. What is more, far more subjects are available who can perform the laboratory tasks than could be found from studies on the job.

The serious disadvantage to the method, however, which must always be borne in mind is that the results obtained might be artefacts of the laboratory situation and the conditions of the experiments. In psychological experiments subjects are often suspicious, they expect to be tricked and will consequently look for clever ways of performing simple tasks. But to some extent this attitude always results when subjects are under observation and know it. This factor is also present, therefore, in on-the-job studies.

Another way of studying the way men who have had no special training search, is simply to study the behaviour of men on the job. This is not a facetious statement ; it so happens that fault-finders are rarely taught fault-finding ; to quote one source " The test students showed very little ability in the planning of trouble-shooting. They stated that they had not received instructions on any particular method for trouble shooting " (Anon 1953). There is no reason to believe this to be an isolated instance.

In these on-the-job studies, however, the secondary difficulties confound the issue. They are reduced in studies such as those by Bryan *et al.* (1956) and Glaser and Phillips (1954) who used simulated ' real ' situations. But with these tasks, knowledge of electronics was necessary and many of the secondary difficulties such as the lack of adequate diagrams were still present. Furthermore all studies of this kind suffer from the limitation already mentioned

that they must be carried out on a highly selected group of individuals. In the future recruitment difficulties are likely to arise and potential fault-finders will be less highly selected. These studies cannot contribute to more general questions such as that of whether a different training problem is likely to be presented when these less intelligent personnel are inducted. Laboratory studies are not restrained in this way.

The third basic question, that of how men should be trained, can only be examined when the first two have been answered. It is discussed, however, at a later point in this paper and some tentative suggestions are put forward.

An account will now be given of the laboratory experiments. These are exploratory rather than conclusive and represent the early work in a research project which is continuing. They are presented in four groups. The first consists of a fairly detailed analysis of the way naïve subjects search for a fault in a complex flow system. In the second the strategies used in simple flow problems and those used in searching for a faulty member of a set of unconnected units are examined. (These tasks are presented under differing conditions which affect the ideal choice of strategy so that it is possible to see whether subjects shift their strategies appropriately.) Some different ways of presenting the task of searching among unconnected units are then compared in the third group. In the fourth, learning in the laboratory situation is studied. A final section contains some data on the way individual differences are related to the choice of strategy.

§ 4. Experiment I : A Complex Flow Problem

The purpose of this experiment was to examine the strategies which naïve subjects would employ when searching in a complex flow system in which the units were equally likely to be faulty and tests were equally difficult to carry out.

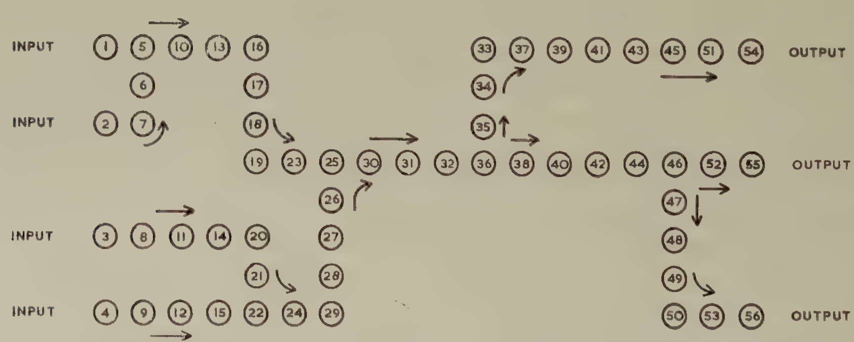

Figure 1. The flow system used in Experiments I and VII. The stages are numbered. Signals flow from the four inputs to the three outputs, the direction of flow is shown by arrows.

Figure 1 shows the structure which was used. There were six problems. In three the structure was displayed exactly as in the figure ; in the other three the layout was changed by removing all the bends so that, for instance, stages 26, 25, 23 . . . 17, 16, 13, 10, 5, 1 were lying all in a straight line. Problems were created by making one of the 56 stages faulty. The effect of such a fault was to make the signal faulty at all points beyond this stage, i.e. from the

faulty stage to the output (or outputs). The faulty stage was either number 9, number 17 or number 51 (see Fig. 1). Each fault was met twice by each subject, once with each layout. The apparatus used in this experiment was very primitive. Each stage was represented by a small square of cardboard. If the signal was correct both when it entered and when it left the stage, the square was marked with two ticks. If the signal was correct on entry but faulty on leaving, it was marked with a tick and a cross. If it was faulty when both entering and leaving, it was marked with two crosses. The squares were placed face downwards. To make a test the subject had to examine a stage, and his moves were recorded by observing which squares he lifted. After examination, each square was replaced face downwards.

There were 24 subjects. Before solving the six test problems, they were given instructions describing the nature of a flow system, and 19 graded familiarization problems which included simple chain structures, structures with two inputs and one output, and structures having one input and two outputs. They were therefore thoroughly familiar with the effects that faults would have upon signal flow.

When stage 9 was faulty all three outputs registered faults. This was shown by colouring the backs of the squares representing stages 54, 55 and 56. The best way of locating this fault would be as follows : since all outputs are faulty the fault must lie in, or to the left of, stage 36 ; thus there are 29 possible loci (the input stages were all marked by colours to show that they were functioning correctly and did not need to be examined) ; by checking stage 23 or 26 these can be divided into two roughly equal groups ; if 23 is checked it is found that the signal leaving it is correct, therefore the chain from stages 1 and 2 up to 23 must be correct ; a second check at 24 would show that the fault lay between inputs 3 and 4 and the stage 24 ; a third check at 21 would show that it must be between 4 and 24, by checking at 15 then at 12 or 9 it would finally be located. Five or six checks would be sufficient according to whether or not 12 was checked before 9. (An element of chance is present.) When the fault is at 17 a good strategy is to begin at 26 and then test 17. This will lead to the fault in two moves, but an equally good strategy would lead to testing 23 then 13, then 18, then 16 and finally 17 which involves five moves. On an average, with the fault at either stage 9 or stage 17 4.4 moves would be required per problem if the best strategy were used ; when stage 51 was faulty the number of moves required to locate this specific fault would be, on an average, 3·3.

4.1. *Results*

The results are considered from three points of view.

4.1.1. *The number of moves taken to locate the fault.*

The average number of moves per problem for the four problems when stage 9 or 17 was faulty, was 7·0, 6·0, 5·9 and 6·5 (in temporal order). In each case this is significantly greater than 4·4, the mean number of moves which would have been required if the best strategy has always been used ($P < 0{\cdot}01$ using a 2-tailed t-test). The greatest number of moves which would be required on any one problem, if the best strategy were used, is six. All but one subject used seven or more moves on at least one problem ; eight subjects used ten or more at least once ; and one subject took fifteen moves on one occasion.

On the problems where only one output was faulty, the number of moves was closer to that which would be required using the ideal strategy. The means were 3·7 and 3·6 which are not significantly different from 3·3, but 8 subjects took more moves than the maximum of four which would have been needed with the best strategy on at least one problem.

The number of moves does not give a very clear picture of the general adequacy of the subjects' behaviour, since it could be dependent upon specific features of the problems which were used. By examining the subjects' strategies it is possible to generalize to other problems and to predict the efficiency with which they would solve them. The strategies are examined in two ways: by looking at the way subjects began the problems, and by analysing all their moves.

4.1.2. *The way subjects began the problems*

The first move in these problems is the most important, since it reduces the number of possibilities by the greatest number. This section is therefore of particular interest. The data have been examined in two ways: Table 1 shows the number of subjects who began with good moves (in the light of the ideal strategy described above); and Tables 2 and 3 show the frequency with which different points were chosen.

Table 1

Number of Subjects making Good Beginning Moves

		1	2	3	4
Frequency of good beginnings		1	2	3	4
Number of subjects:	fault at 9 or 17	9	0	0	1
	fault at 51	13	6	–	–

From Table 1 it can be seen that only one subject began all the four problems when the fault was at stage 9 or 17 in the best way, and only six did so on both of the other problems. When Table 2 is examined what is most striking is that 85 of 96 problems were begun at one of six points, 73 of them at points which left the number of alternatives (it began at 29) as large as or greater than 24. The two most commonly chosen points were the junctions (stages 25 and 36); nearly half the beginning moves were at these two points. Most of the stages at which subjects began were in the middle of the complete array, but, of course, this was not the important factor in these problems. It might mean, however, that the subjects were attempting to halve the possible loci, but were misled by the display.

Table 2

First Checks on Problems with the Fault at Stage 9 or 17

Stage	Number of subjects beginning at a given stage: 1st problem	2nd problem	3rd problem	4th problem	All problems
23 or 26	1	4	3	4	12
25	4	7	11	5	27
30	5	4	5	4	18
31	3	1	0	0	4
32	4	3	1	3	11
36	6	4	3	4	17
22	0	1	1	1	3
19	0	0	0	2	2
16	1	0	0	0	1
28	0	0	0	1	1

With the fault at stage 51, subjects tended to begin either at the middle of the chain or near the input. The first stage in the chain (stage 35) was chosen 11 times out of a possible 48. The way of beginning this problem can be compared with the way subjects began their first two familiarization

problems (which were also straight chain problems). When this is done it is seen that the stage in an equivalent position to stage 35 was never chosen ; the difference is significant at the 1 per cent level using McNemar's (1949) test. This difference means that a chain which is part of a larger structure is not responded to in the same way as an isolated chain. In this case it would seem that those subjects who checked stage 35 were confirming the deduction, which they must previously have made from the state of the outputs, that only this one chain was relevant.

A final general point about this data is that although comparatively few beginning moves were as good as they could have been, only four were completely useless (see Table 3).

Table 3

First Checks on Problems with the Fault at Stage 51.

Stage	Number of subjects beginning at a given stage	
	1st problem	2nd problem
33	0	5
34	1	2
35	6	5
36*	1	1
37	1	2
39	8	5
41	4	3
43	0	0
45	2	0
51	0	0
54	0	0
Other*	1	1

* These stages are not in the faulty chain.

4.1.3. *The kinds of checks the subjects made*

After the first move the problem was no longer the same for each subject ; for the remaining alternatives depended upon where the first check was made. Because of this it was not possible to consider all the subjects together. Instead each of the later moves has been considered in the light of the alternatives from which it was chosen, and classified. By examining the frequency with which different kinds of move were made, some idea can be gained of the strategies employed.

Eleven kinds of move have been distinguished, although they are not all mutually exclusive categories. The first nine categories can be considered to describe poor moves ; the last two categories, good moves. Grouping in this way reveals that there were altogether 406 poor checks and 383 good ones. The data is shown in Table 4.

There were few errors or repetitions. The number of errors shown can be a little misleading since they were not independent of each other. What happened was that some subjects began to move in the wrong direction, as if they read tick for cross or vice versa when they made their checks. They sometimes did this for a number of moves. Some subjects seem to be more prone to make reversals of this kind than others as can be seen from Table 5.

Wastage of moves would seem to be largely due to the tendency to check junctions and to move in small steps from a point already checked. A check made at a junction often yielded very little information, since if two input

chains led into it the check would not show which was faulty. The subjects must have been aware of this because they had met problems of the same kind during their familiarization series. The tendency might have been due simply to the perceptual prominence of junctions or it might have been that subjects chose these points because these checks could be remembered more easily. Other results, to be discussed below, suggest that the first of these reasons is most likely the correct one.

Table 4

The Classification of all Checks made in the Solution of All Problems by 24 Subjects. If a Check falls into more than one Category it is counted in each. A Correction is given for the number counted twice. (None were counted more than twice.)

Category/Description	First 3 problems	Last 3 problems
Confirms a deduction	23	17
Checks at a junction when it would be better to check to next to it (i.e. checks 25 instead of 23 or 26)	49	46
Checks a junction at other times	22	16
Checks at a stage adjacent to one already checked	68	37
Checks close to but not adjacent to a stage already checked	27	31
Checks a stage in an input chain before ascertaining that there is a fault in the chain (e.g. beginning a problem with 3 faulty outputs at 28 or 17)	39	37
Makes a check that cannot yield information (e.g. begins a problem with 3 faulty outputs at 40)	13	24
Repeats a check	5	1
Avoids the above listed kinds of moves but does not make the best possible check	43	41
TOTAL	289	250
NUMBER COUNTED TWICE	63	70
Good. A check that would be made in the light of the best method (as described in the text)	109	130
Good, but restricted in so far that no alternative move could be compatible with the evidence already available, e.g. checking 9 after a check at 15 has shown the fault lies between 4 and 15	65	79
TOTAL	174	209

Table 5

Reversals.

Frequency of reversals made	0	1	2	3
Number of subjects	13	5	4	2

Although it is dangerous to emphasize particular results, an idea of the way some subjects were attracted to junctions can be given by quoting the actual moves made by one subject. The fault was at stage 17 and this subject's moves were : 36, 25, 5, 22, 23, 10, 16, 19, and finally 17 (this subject also shows a tendency to move in small steps from points already checked).

Moving in small steps yielded little information, it reduced the number of possible loci by only a small amount. It could be that these moves were regarded by the subjects as slow but sure. They were sure in the sense that they were likely to give the same results as the previous check and hence confirm it. Alternatively the opposite could have been the case ; the subjects

might have argued, when they took a small step, that, if the faulty stage were encompassed by the step, then it would have been located very quickly. This second interpretation equates this kind of move with gambling.

Checks which were quite clearly confirmatory, such as beginning moves at stages 36 or 35 (according to the problem), were less common than most other kinds. That they occurred as frequently as they did is perhaps surprising, since the simple deductions involved in the task were very rarely made incorrectly. As mentioned above, on only four occasions did any subject begin by checking a stage which was irrelevant to his problem.

One kind of confirmatory check is of special interest. Sometimes, where two input chains led to a junction, a subject checked the ultimate stage in one chain and then the ultimate stage in the other (viz. beginning, when the fault is at 17, by checking 26 and then 23). Twenty-one subjects were in a position to make this kind of confirmation at least once ; of these, 12 did so on one or more occasions. The interest in this phenomenon is that it shows how these subjects, after eliminating some possibilities, fail to see the remainder as an independent group. Instead of re-assessing the situation (after it has been found that the signal leaving stage 26 is correct, the remaining stages from 5 through 10, 13, 16 . . . to 36 are best considered as a simple chain) they seem to have been bound by their earlier way of looking at the problem (viz. stage 25 is still regarded as a junction, and stage 23 as a member of a limb).

The analysis presented here has the disadvantage that it does not show any strategies as a whole. It is clear that the best strategy was not used frequently. In fact only one subject used 'good' moves throughout a problem where the fault was at 9 or 17. Nine subjects used good moves throughout one of the other problems (with the fault at 51) ; four of these did so for both. While it is possible to separate out these instances it would be impossible to classify the other strategies since they were so varied.

The data has been analysed for evidence of improvement. Table 4 shows, separately, the checks made in the first three problems and those made in the last three. (The experiment was designed to permit this comparison.) It can be seen that more good checks and fewer poor ones were made in the second half of the test than in the first half. This difference, examined per subject, is not statistically significant.

Subject differences : Twelve of the subjects were Naval Ratings, the other 12 were members of the staff of the A.P.U. The total number of moves made by each group was not very different, although the laboratory group required slightly fewer. More members of the laboratory group used the best strategy ; seven used it at least once, whereas only two of the ratings ever did so. This difference, viewed as the difference between two binomial proportions, is significant ($p < 0{\cdot}05$).

4.2. *Summary*

To summarize very briefly : In a complex flow problem naïve subjects did not use what is theoretically the best strategy of searching. They took more moves than they need have done because of their inefficient technique. They seem to have been influenced by perceptually striking features of the display ; this might be due to the value of prominent points as aids to memory

but later evidence (see below) does not favour this interpretation. Where deductions were necessary in the initial phase of the search they were rarely made incorrectly ; but subjects commonly confirmed them with an otherwise redundant check.

This experiment was first reported in somewhat different form (Dale 1955). An experiment in many ways similar to it has been reported by Goldbeck *et al.* (1957), but this communication contains no description of strategies. Goldbeck *et al.* were mainly interested in the effect of instructing subjects to use the half-split technique (this is the American term for dichotomy, see Miller *et al.* 1953 a, b) and their results will be mentioned below. A point of interest is that by using a particularly confusing display they were able to upset their subjects' performance in the initial, deductive, phase of the search.

§ 5. Experiment II : A Simple Flow Problem Compared with a Search among Unconnected Units

The purpose of this experiment was to compare the strategies subjects employed when searching for a fault in a simple flow task under various conditions, with those they employed when searching for one of a number of independent units under similar conditions.

The conditions varied were :

(1) The presence or absence of an aide-mémoire. In one condition the subjects had to remember their moves as in Experiment I ; in the other condition they were able to record them.

(2) The relative probabilities that units would be faulty. There were altogether seven problems for each subject ; subjects were paired, and for one member of each pair the same unit was faulty for problems 2 to 7 ; it was so arranged that the other member of the pair would take roughly the same number of moves to locate a ' fault ' which had not been pre-assigned in this way. Thus for half the subjects one unit was more often faulty than the others.

(3) The time allowed. There was no time limit for the first five problems. In these, as in Experiment I, subjects were instructed to locate the fault in as few moves as possible. On the last two problems, however, a limit of two minutes was set, and subjects were warned that, if the fault had not been located, they would be stopped at the end of this period.

Table 6 shows the number of subjects in the different groups. (All subjects solved seven problems, the last two being under a time limit.) Eighty fresh subjects were used in this experiment.

Table 6

The Design of Experiment II

	Flow Task				Unconnected units task	
	Sockets represent stages		Sockets represent test-points		Sockets represent equipments	
	Record of moves	No record	Record of moves	No record	Record of moves	No record
Equal probabilities	5 subjects	5 subjects	5 subjects	5 subjects	10 subjects	10 subjects
One part more often faulty than the rest	5 subjects	5 subjects	5 subjects	5 subjects	10 subjects	10 subjects

A special apparatus (see Fig. 2), which is described in detail elsewhere (Dale and Brown 1958), was used for this experiment. It was constructed so that it could be used to present a large variety of searching tasks. As in the real job, there is an ' equipment ', and test gear. (A signal generator can also be introduced.) The test gear is simplified to provide an all-or-none, i.e. ' good ' or ' bad ' indication. In order to make tests dials first have to be turned to particular settings ; each dial is set by turning a screw which moves the dial through a low-gear worm drive. The subject is provided with a screwdriver with which to turn the screws. After setting the dials a lever is depressed to make the test. Depressing the lever returns the dials to their resting position so that in order to make a second test they must be reset. The test gear can be set in various ways, some settings require only a little effort (i.e. the dials do not have to be turned very far), others require a considerable amount of effort. (The test points on the ' equipment ' are marked to indicate the setting required.) The subject works alone in a cubicle. Outside the experimenter can see from a display which test the subject is making. He controls the result of each test and thus can manipulate the situation to a considerable extent. In this experiment the effort was held constant at a low level ; it took about 15 sec to set up the test gear to make a check.

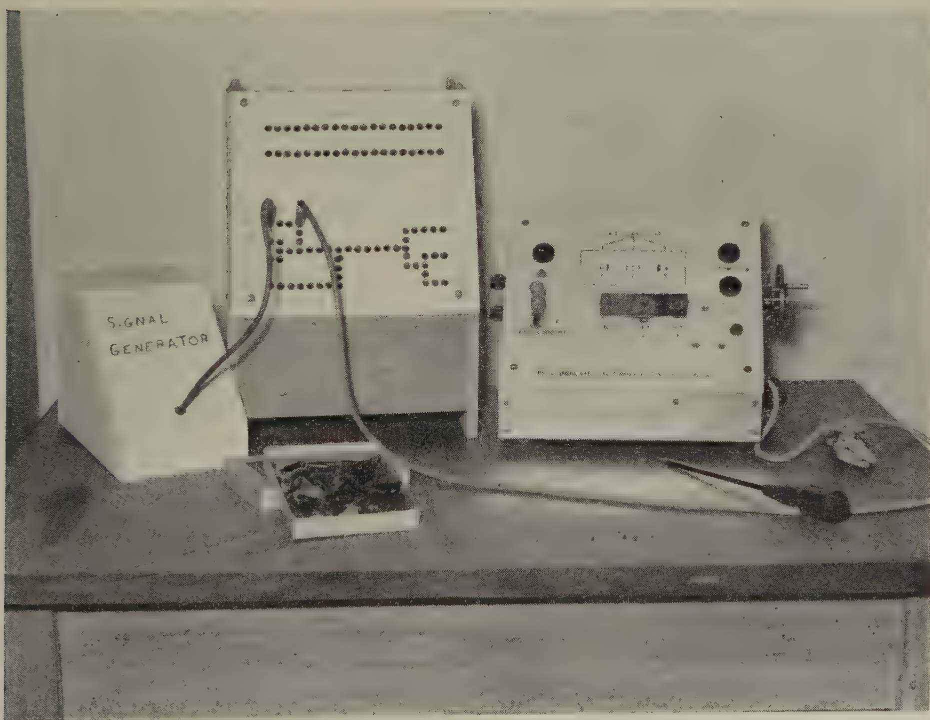

Figure 2. The apparatus used in Experiment II as seen by the subject : The display, which represents the equipment or equipments, is on the left ; the test-gear on the right. The pegs in the tray in the foreground can be plugged into the sockets to record checks which have been made ; they are coloured red or green. (The signal generator was not used in the experiments reported here.)

The display was the same for all problems except for minor changes. It consisted of a row of 19 sockets which could be tested by plugging in a jack plug and operating the test gear. In the simple flow task the left-hand end was marked ' input ', the right-hand end ' output ' and, as in Experiment I, subjects were instructed that it represented an electronic equipment and were told about signal flow. The task was presented in two ways. In the first, the sockets represented the actual stages and as in the Experiment I the faulty stage could be recognized by a single test (the test gear was made to indicate *both* ' good ' and ' bad ' simultaneously when the faulty stage was examined). In the second, the sockets represented test points and the faulty stage was located between a ' good ' point and an adjacent ' bad ' point. With this second method of presentation subjects were given a special tester with which they could check a stage as opposed to a test point. They were told to use it only to confirm the location of the fault ; but, as can be seen below, they sometimes used it prematurely. In the searching task where the units were independent, subjects were told that the sockets represented a batch of separate equipments, one of which was faulty.

5.1. *Results*

5.1.1. *The simple flow problem*

Three ' pure ' strategies have been distinguished, and two recurring mixed strategies. These are described in Table 7 which shows the frequency with which they were used. The data is pooled to some extent in this Table, because the recording or non-recording of moves, and the different ways of presenting the task, did not lead to significant differences in behaviour. (The data from the subjects who recorded their moves are strikingly similar to those from the other subjects.)

Table 7

The Number of Subjects using each Strategy in the Simple Flow Problem

Strategy	1st problem	2nd problem	3rd problem
Dichotomizing (successively halving the array)*	8	11	12
Bracketing	5	4	3
Moving in steps from a point already checked :			
All steps (up to six units)	15	13	13
steps of 3 units or less	12	10	8
steps of only 1 unit	7	2	1
Dichotomizing at first and then stepping	3	3	2
Bracketing and then stepping	1	1	1
Other strategies	8	8	9

* 6 subjects dichotomized on all 3 problems. 25 did so at least once in the complete series of 7 problems.

A strategy which has not previously been described was used by some subjects. This has been called bracketing. It is similar to dichotomizing but includes a redundant move. In a chain of 19 items an example of bracketing would be the series of checks : 10, 19, 14, 17, and 16. Sometimes the first two moves would be followed by stepping ; this is described as ' Bracketing and then stepping '.

Only the data from the first three problems is shown in Table 7. All subjects had been treated alike up to this point with regard to probabilities. When subjects began their fourth problem, however, some had evidence that one stage had been faulty twice. If this is treated as evidence of differential probability then, for these subjects, the ideal strategy would be to begin at a point between the middle and the twice-faulty stage. (The often-faulty stage was always close to one or other end of the chain so that it would be prominent.)

The tendency to move along in steps was quite strong ; on the third problem 13 subjects stepped and three others used stepping as a partial strategy (see Table 7). This result is comparable to that of Experiment I, which showed a similar tendency. Stepping cannot be the result of an attempt to avoid forgetting, since as many of those subjects who recorded their moves adopted this kind of strategy as did those who had to remember them. A possible reason for this behaviour is that these subjects failed to clearly distinguish this task from that of searching among unconnected units. This assumes that the stepwise strategy would be adopted quite generally in the other task (where it is the most sensible strategy) and that these are the only subjects who fail to see that it is inappropriate here. Evidence cited below, from the unconnected units task, however, indicates that this is an unlikely explanation.

This data contains evidence of learning. More subjects dichotomize, and fewer proceed in small steps. on the third problem than on the first. Examined subject-wise by McNemar's (1949) technique, the reduction in the number using small steps is shown to be statistically significant ($p < 0{\cdot}02$). On later problems still more subjects dichotomize (regardless of the fact that for half of them it is no longer the best strategy). The numbers are 15, 21, 18 and 18, on the 4th, 5th, 6th and 7th problems respectively. The increase from the first to the last problem is statistically significant ($p < 0{\cdot}02$ using McNemar's test *loc cit*). On the last problem, 8 of the 18 subjects who dichotomized had experienced unequal probabilities ; this clearly had not greatly affected the overall number choosing this strategy.

When one stage was repeatedly faulty, some subjects adjusted their strategy so that they began closer to this stage. On the 5th problem, three of the twenty subjects responded to it in this way ; on the 7th problem, eight did so (this difference is not statistically significant ; $p > 0{\cdot}05$). This increase, if real, could have been due either to the time limit set on the last problem, or to the fact that, after observing more instances, the subjects had stronger evidence that this stage was more likely to be faulty than the others. Theoretically the provision of a time limit favours the use of a strategy in which the most probable parts are checked first. Our results suggest, since only 8 of the 20 showed any influence, that only some subjects were aware of this fact. But the evidence is not conclusive since it can be argued that the time limit was not sufficiently stringent. Some subjects, in fact, commented that two minutes provided them with plenty of time for a thorough search. The other group, who had no evidence of differential probabilities, were unaffected by the time limit.

Those subjects for whom the sockets represented test-points had to confirm that they had successfully located a faulty stage. They did this by using

a special tester. It took considerably longer to operate this tester than to make an ordinary check. A confirmatory test in this way parallels the removal of a sub-unit in the real job ; it pays not to do it until sure that it will give the expected results. Nine out of 19 subjects used this tester prematurely (i.e. before they had sufficient information to be sure of the locus of the fault) on at least one occasion, despite instructions to first make sure they had located a faulty stage. (The twentieth subject used it prematurely because he had a special hypothesis about the way the problems were being scored.) This behaviour was not influenced by the time limit ; six of the nine made premature confirmatory tests only when there was *no* limit.

5.1.2. *Searching among unconnected units*

Here again the first three problems have to be considered separately. Three kinds of strategy have been distinguished : (1) Stepwise—beginning at one end checking each unit in turn ; (2) Pattern—following some more complex order such as checking the first then the last, then the 18th, then the 2nd and so on ; (3) Random—following no perceptible system.

When the fault was located rapidly there was so little data that in some cases it was not clear whether the subject was using a Pattern or a Random strategy. A fourth, ' Other ', category has been used for these instances.

Table 8

Searching among Unconnected Units : the Number of Subjects Adopting each Strategy

Strategy	1st problem	2nd problem	3rd problem
Stepwise	17	9	9
Pattern	9	14	12
Random	14	12	12
Other	0	5	7

Recording moves again made no difference and the results of those subjects who recorded their moves and those who did not, have been pooled. (Table 8 shows the number who adopted each strategy.) These data show an apparent deterioration, since the number of subjects using the straightforward, Stepwise, strategy is lower on the later problems than on the first one. Some of the subjects, however, stated in retrospective reports that they adopted this strategy on the first problem because they believed then that they were obliged to use it. The results of the first problem, therefore, give a misleading impression. The number in the ' Other ' category is zero on the first problem because, on this problem, all subjects were forced to take 10 moves, and enough data was therefore obtained to distinguish between Pattern and Random strategies.

It is clear from Table 8 that comparatively few subjects used the Stepwise strategy. Excepting the first problem, there were more subjects working with the Random than with the Stepwise strategy.

One implication of this result is that the tendency to move along in steps noticed in the Simple Flow experiment is unlikely to be due to a failure to distinguish between that task and this one. It was not a general tendency here. But the criterion of Stepwise is different in each case and it might be argued that these subjects would perform both tasks in the same way. If the data is re-analysed and a broader category of Stepwise defined to include

those subjects who moved in from one or both ends in steps of three or less units, it is found that 24 subjects used this strategy on the first problem, 18 did so on the second and 11 on the third. The number of extra cases included, therefore, by this change of definition, is not great. Later evidence (see below) shows that those subjects who proceeded in unit steps in this task were the more intelligent ones, whereas those who 'Stepped' in the flow problem were the less intelligent. Thus subjects who use the strategy in one task differs from those who do so in the other (see Dale 1958 c).

Table 9

Searching among Unconnected Units : the Number of Subjects Adjusting their Strategies in the Light of Probability Evidence ($N=20$)

	Problem			
	4th	5th	6th*	7th*
Inspecting the more probable unit early in the search	11	15	17	16
Inspecting the more probable unit on the first move	3	7	7	6

* There was a time limit on these problems.

On the later problems, where one unit was more often faulty than the rest, most subjects adapted their strategy so that they inspected this unit early in their search ; a few began by checking this more-probable unit (see Table 9). There was a tendency for those adapting completely to be subjects who had previously worked at random at least once, but this was not statistically significant. The time limit clearly did not affect the issue (see Table 9) and with this task, as opposed to the Flow task, it cannot be objected that the time allowed was sufficient for a thorough search. A complete search would have taken, on average, over 5 minutes.

The time limit had no effect on the other subjects (those for whom the fault might have been at any point). There was no tendency, under time pressure, for those using the Stepwise strategy to discard it, or for any other subjects to change their strategies.

§ 6. Experiment III : The Effects of Varying Relative Probability Alone and of Varying Both Relative Probability and Relative Effort

In this experiment 18 fresh subjects were given the task of searching among a set of unconnected units for one which was faulty, under differing conditions. The apparatus and display were basically the same as those used in Experiment II.

Relative probability was varied by marking some units distinctively and instructing subjects that the marked units were more likely to be faulty than those not marked. Here, as in Experiment II, most subjects (14 of the 18) began by checking first those units which were more likely to be faulty.

When both probability and effort were varied, no such clear effect was observed. Effort was varied by demanding different settings of the test gear (see above). Nine different levels of effort were used ; the greatest demanded 12 times more work than the least, and most subjects required about 5 minutes to make a test with this (most effortful) setting. It was expected that subjects would begin by testing those units which were more likely to be faulty and those which were easy to test, but the way these two factors would be weighted could not be predicted. Of the 18 subjects, one checked the more

probable units first, regardless of effort, and then checked through from the easiest to the most effortful. Three others began with the more probable but then disregarded effort. Three more took some account of effort, and began with easier tests. The other 10 subjects showed no sensible regard for effort or probability, and some even made the most effortful checks first.

Detambel (1956) and Detambel and Stolurow (1957) have shown, in a similar situation, that the probability variable weighs more heavily than the effort variable in determining subjects' choices. In both these studies only a small number of units (3 in one case, 4 in the other) were used, and the subjects learned about effort and probability through familiarity with the material (as in Experiment II).

§ 7. Experiments IV and V: The Effect of Varying the Method of Presentation

The aim of the two experiments to be reported was to see whether the strategies employed when subjects search among unconnected units would differ if the task was presented in different ways.

Two new ways of presenting the task were used. In the first, the letters of the alphabet were used as the units; in the second, the apparatus used in Experiment II was slightly modified, and the 19 units were 19 wireless transmitting stations which the subjects had to monitor. Eighty-four new subjects were used in these experiments.

7.1. *Experiment IV: Searching in the Alphabet*

There were three variants of this task. The first group of subjects were told by the experimenter "I am thinking of a letter of the alphabet and I want you to discover which one it is with as few guesses as possible." The subject was told after each guess 'No!' if his guess was incorrect or, when he successfully located the desired letter, "That's it". No other information was given. The second group had similar instructions except that the word "questions" was substituted for "guesses". The third group were handed a pack of 26 cards each bearing a different letter, they shuffled the cards themselves, then the experimenter cut and instructed them to discover which one was at the bottom of the pack. (Again the wording did not imply that they should guess.) Subjects were given two, or in some cases three, problems. The results considered are for the first problem only; very few subjects changed their strategy for the second problem.

7.2. *Experiment V: Wireless Monitors*

The 19 sockets were numbered to represent wireless transmitting stations. When one was tested (using the test-gear) the faulty–correct indicating lamps flashed a short message which was repeated at brief intervals. The subjects were told to locate as quickly as possible the one station which was transmitting a given message. The messages were all rather similar and subjects were warned that they must be sure to get the correct one.

7.3. *Transfer*

Those subjects given the wireless monitoring task were given the alphabet task immediately afterwards (it was presented on cards) so that it is possible to compare the strategies they adopted on each.

Table 10

Searching in the Alphabet and Searching for a Wireless Station

	Stepwise	Pattern	Random
Searching in the alphabet :			
' guesses ' instructions $n=26$	2	3	21
' questions ' instructions $n=12$	0	0	12
using cards $n=27$	2	1	25
Searching for a wireless station :			
results of 3rd problem $n=18$	7	8	3

Table 11

Strategies used for the Alphabet and Wireless Monitoring Tasks Compared

		Searching in the Alphabet	
		Stepwise or Pattern	Random
' Wireless Monitoring '	Stepwise or Pattern	2	13
	Random	0	3

7.4. *Results of Experiments IV and V*

The results of these two experiments are given in Tables 10 and 11. It is clear that there was a strong tendency to adopt a Random strategy when searching in the alphabet, whichever variant of the task was used. On the Wireless Monitoring task the systematic strategies were preferred, and fewer subjects worked at random than would be expected from the results of Experiment II. This could be due to the selection of subjects, but the transfer experiment clearly shows that the same subjects shift to different strategies when given the alphabet task (Table II). (The difference is highly significant, $p<0{\cdot}001$.) Some characteristic of the Wireless Monitoring task might, therefore, lead to this difference from the result of Experiment II.

Bryan *et al.* (1956) provide supporting evidence for shifts of the kind observed here. They noted that many men who were given two kinds of problem, one in radio equipment the other in radar equipment, did not approach both of them in the same way. Clearly the logical requirements of a task do not determine the strategy which men will use.

§ 8. Experiments VI and VII : A Further Investigation of Learning

Some of the data already presented shows that when subjects are given a number of problems there is a tendency for them to use more appropriate strategies on the later ones. Two experiments are reported here : in the first, further evidence of learning in the task of searching among unconnected units was sought ; in the second, the success subjects obtained with different strategies in a complex flow was manipulated and the effects observed. Some data is also presented which shows the effect of success upon searching among unconnected units.

8.1. *Experiment VI : Changes in Strategy over a Long Series of Searches among Unconnected Units*

Table 12

Changes over a Long Series of Searches among Unconnected Units : the Number of Subjects Adopting each Strategy

Strategy adopted	Stepwise	Pattern	Random	Other
First problem	0	1	8	1
Tenth problem	0	7	2	1

A small group of 10 subjects was given 10 problems of this kind, with the display used in Experiment II. Table 12 shows the strategies used on the first and last of these problems. The shift from the use of the Random strategy to the Pattern strategy is statistically significant ($p<0{\cdot}02$ using McNemar's test). Thus there was a tendency for subjects to become more systematic, but no subjects changed to the Stepwise strategy.

8.2. *Experiment VII : The Effect of success in a Complex Flow Task*

It is reasonable to suppose that if a strategy leads occasionally to rapid success it is likely to be used again. If it never does so then it is more likely to be discarded. In a flow task, any strategy other than dichotomizing is risky. If a check divides the alternatives unequally and the fault is in the smaller group, success is more rapid than with dichotomy. Rapid fault location sometimes occurs, therefore, when poor strategies are used. In this experiment, the experimenter so manipulated the situation that if the alternatives were divided unequally the subject would always be unlucky. In these conditions the best strategy *always* led to the fewest moves.

Twelve subjects in an experimental group were given 14 problems, of the kind used in Experiment I, which were all rigged by the experimenter. Twelve subjects in a control group were given 14 similar problems ; the first two and last two of these were rigged, but in the other 10 the fault was fixed beforehand ; these subjects, therefore, had a normal run of luck. It can be seen that the first and last two problems were the same for both groups, and could be used for comparing their performances. The number of moves made in these four problems give an indication of the effectiveness of the subjects' strategies.

Table 13

The Effect of Success in a Complex Flow Task

	Number of moves taken per problem	
	before ' training '	after ' training '
Experimental group :	8·92	8·50
Control group :	9·93*	8·08*

* Tested per subject this difference is significant ($p<0{\cdot}05$, 1-tail test).

It was expected that the experimental group would improve to a greater extent than the controls, but, as the Table 13 shows, the opposite was the case. The control group improved slightly more than the experimental group. Considerable room for improvement remained for both groups, since, had the best strategy been employed, on the average 4·2 moves would have been required per problem. (In all of these problems the faults were such that all three outputs were faulty.)

8.3. *Success, and the Strategies Used for Searching among Unconnected Units*

Sometimes when subjects used a Random strategy they got into a muddle; they repeated moves, and sometimes took more moves than there were units to solve a problem. (One subject, for instance, took 36 moves to find a letter in the alphabet.) Data from some of the experiments already described has been analysed to see whether such experiences led subjects to change their ways and adopt systematic strategies. Twenty subjects were found to have become muddled in this way at least once. Of these, 12 used the Random strategy again at least once, whereas 8 changed to a systematic method of searching.

The fact that two thirds were not influenced by this experience suggests that, just as the gambler does not usually give up when he loses a bet, these subjects were prepared to take the rough with the smooth.

8.4. *Summary*

To briefly sum up this section: subjects improve their strategies when they are given a series of problems but, in the series studied, considerable room for improvement remains. This is true of both the flow problems and the searches among unconnected units. It is possible that further improvement would be observed if the subjects were exposed to longer series of problems.

With both kinds of problem the improvement does not appear to be related in a simple way to success and failure. Faust (1958) has obtained a similar result to this in a study of the game 'Twenty Questions'.

§ 9. Individual Differences

Intelligence test scores were available for all the subjects in Experiment II and this data was subsequently examined to see whether choice of strategy was related to intelligence. It was found that, in both tasks, the subjects who adopted the appropriate strategies (dichotomy in one case and the Stepwise strategy in the other) were significantly more intelligent than those who did not. Details of the analysis are given elsewhere (Dale 1958 c).

This data was also examined to see whether strategies could be related to previous experience. Although most of the subjects in this experiment were naïve some had received training in fault-location in various kinds of equipment; one, for instance, on internal combustion engines; another on armoury. When the men were classified into those with technical and those with non-technical backgrounds, however, no correlation with their strategies was detectable. It must be added here that some men had been trained to use inappropriate methods. One, who Stepped in the separate-units task, said that he had been taught to work along a chain when tracing faults in engines. (Thus he used appropriately a strategy which had been taught inappropriately.)

The results of Experiment I support this evidence that intelligence is related to strategy. A higher proportion of the subjects recruited from the laboratory used the best strategy than did the Naval Ratings. (It is reasonable to assume that there were differences in intelligence here.)

Analyses of both the Alphabet and the Wireless Monitoring tasks show much smaller correlations, which are not significantly different from zero. The reason for this is not known, but in the case of the Alphabet task it would seem that there is a tendency to treat the task as a game which affects all subjects, possibly against their better judgment.

Saltz and Moore (1953) found no correlation between on-the-job performance of fault-finders and their intelligence, but, as they point out, this is most likely due to the fact that their subjects had previously been selected on the basis of intelligence. All studies using trained or trainee engineers suffer from this limitation.

§ 10. Discussion

10.1. *The Theoretical Interest of the Results*

Many facets of the behaviour observed in these tasks are familiar to students of problem solving. Three points have been singled out for discussion. These are (1) the correlation observed between choice of strategy and intelligence; (2) the problem of explaining the learning which was observed; (3) searching viewed as risky decision-taking.

That brighter subjects use better strategies than dull ones is to be expected, since problem solving ability and intelligence are often considered to be synonymous. The observation made above is not tautologous, however, since the intelligence tests which were used had a high verbal ability content, i.e. they were not simply alternative tests of problem solving ability. In the tasks of searching among unconnected units this relation is particularly interesting because in this case the best procedure was the simplest one. Here the less intelligent subjects made the task unnecessarily complicated for themselves. Clearly the ability of the bright subjects was shown by their initial analysis of the demands of the problem.

The fact that with varied presentation varied behaviour resulted, shows that subjects in general do not analyse the tasks and assess the strategy which is appropriate on the basis of their logical requirements. Or, at any rate, they do not do so correctly. The process by which they decide that a task should be treated in a given way is likely to be complex. It clearly depends upon their repertoire of responses as well as their analysis of the situation. But the Transfer experiment (Experiment V above) provides evidence that at least some subjects' reactions are not limited by their repertoire of responses. Some of those who searched randomly in the alphabet had, just before, been searching systematically in the Wireless Monitoring task.

Apart from the influence of perceptual factors in the initual approach to a task, there is evidence which suggests that these also affect particular choices. The tendency to check junctions in Experiment I is most probably due to their perceptual prominence. This interpretation is supported by Baddeley's (1958) results which show that when a simple array, of the kind used in Experiment II, has a right angle bend in the row of sockets, subjects frequently respond to this irrelevant cue by making their first check at the bend.

The evidence of Experiments I, II, VI and VII shows that subjects improve their strategies if they are given a number of problems in the laboratory. It is a little difficult to see what governs this learning. Success and failure usually provide the learner with an indication that there is room for improvement and also enable him to discover which changes of behaviour will lead to greater success. In these experiments, the subjects knew how many moves they had taken on each problem ; indeed the experimenter would say after each : " That took x moves. Now see how few moves you need to locate the fault in this next problem." But they were not told how many moves they need take, nor whether their strategies were appropriate. Under these conditions learning would not generally be expected. And even if they had been given targets, rapid learning would be unlikely since the chance element was large compared with the overall difference to be expected from many of the possible changes of strategy. Judging the effectiveness of a strategy is indeed a complex problem for which statistical procedures would be usually invoked.

In the experiment in which success was controlled, the experimental group was in a position in which much of this random element was eliminated, so that the effect of an improvement in strategy was readily apparent. From this ' detection ' point of view then, as well as the reward point of view, it is surprising that they improved less than the control group. As an interim interpretation it can only be suggested that subjects came to revise their assessment of the tasks, but this really only begs the question.

One way of looking upon searching is to regard the choice of each check point, or alternatively the choice of one strategy rather than another, as a risky decision. In the flow problems, checks near the middle carry low risk, because the same amount of information will be gained whatever the result, but those near to one end carry a high risk, since if one result is obtained a lot of information is gained whereas if the other is obtained the check will have revealed very little. In the search among unconnected units risk arises because of the possibility of forgetting. The subject who uses a Random strategy will locate the fault rapidly if his hunches are correct (it is assumed that his choices are governed by subjective probability and other evidence supports this view, see Dale 1958 f). If his hunches are false, and he has to search for a long time, he runs the risk of forgetting moves and becoming muddled. Thus the Stepwise strategy can be regarded as slow but sure, whereas the Random strategy might lead to rapid success or dismal failure.

In the flow problems risk is confounded with redundancy ; Stepwise moves can be considered either as risky or redundant, and the issue is not very clear. Furthermore, it is likely that dichotomy was not considered at all by many subjects. The choice of strategy in the search among unconnected units might well be considered from the risk-taking point of view, however, since many subjects showed that they were aware of the Stepwise approach but rejected it. The risk in this situation is unreasonable. The subjects really stood to lose only by working at random, since the Stepwise strategy is just as likely to lead to rapid location of the fault as any other strategy. But it is well known that people are commonly prepared to accept poor risks ; the popularity of everyday forms of gambling is evidence of this. When real evidence about differential probabilities was provided in this task it was

accepted and used readily. Thus there would seem to be a general willingness to accept probability evidence. It does not seem to be altogether unreasonable to suggest that this might have generalized to subjective probability evidence (hunches).

10.2. *The Practical Significance of the Results*

If these results can only be interpreted as a record of the behaviour of persons within a psychological laboratory they are of no practical significance.

It might be argued that those subjects who used Random strategies when searching among unconnected units did so because they expected to find some catch in the experiment. But this is unlikely to be the whole answer, if even a part of it. For one thing, if this were the case why should the more intelligent subjects be less likely to work in this way? For another, there is the evidence that this sort of behaviour occurs on the real job. Crowder (1954) remarked upon it, and elsewhere Rosen (1957) has given a vivid account of it.

The fact that behaviour was influenced by the way tasks were presented can also be interpreted as a criticism of the laboratory studies. The results of the experiments differed according to the method of presentation. Which, then, was the true analogue of the practical task? This is only a criticism if it can be asserted that the practical task is always presented in one way. What is more likely true is that what has been discovered to be a relevant variable in the laboratory is also relevant in 'real' life. It is unlikely that all men on the job look upon it in the same way and these results suggest that we can influence their strategies by changing their viewpoint. However further analysis of the differences observed are necessary before the key variables can be identified. All that can be safely concluded from the experiments reported is that presentation affects behaviour.

What these results do show clearly is that we cannot expect men to use the best strategies of searching without training; they use them neither in very simple problems nor in more complex ones. The results cannot, therefore, be ascribed to the simplicity or the complexity of the problems used. From the practical point of view it might seem that the criteria of 'good' and 'poor' strategies which have been adopted here are unduly severe. But in Experiment I subjects took about 50 per cent more moves than they need have done because of the inefficiency of their strategies.

The laboratory tasks differed from the real job in that, in most cases, they did not have these two levels of search which are encountered by the electronics fault-finder (who has to isolate first the faulty stage and then the faulty part). For effective fault-finding in electronics it is of prime importance that the search in the flow system should be *sufficient*. The serious wastage of effort noted by Crowder (1954) and Dale (1958 a) resulted when subjects went to the second level of search prematurely; when they checked within a stage which was not faulty. A direct comparison with this kind of behaviour was possible in only one experiment (Experiment II), where it was found that, as in the real job, specific tests were made prematurely in a good number of cases. If it ensures correct identification of the faulty stage, some redundancy when searching in a flow system might be a good thing, as the writer has

suggested elsewhere (Dale 1958 a). Its cost is certainly much less than the cost of chasing a false lead to the point of making tests within a stage which is not faulty.

In the task of searching among unconnected units, the strategy of exploring according to some pattern is logically as good as that of beginning at one end of the row and taking each in turn. Only the Random strategy is seriously inefficient. Some subjects who followed patterns, however, made them progressively more complex, and it might well be predicted that, had they been forced to explore nearly all the alternatives, they would have forgotten their moves. In this task and in the others more subjects might have used the better strategies had they been forced, before they began, to think out all possible ways of proceeding. This is suggested by the evidence of Moore *et al.* (1955). The evidence, however, is not strong.

The fact that a man's strategy depends, at any rate in certain circumstances, upon his intelligence is of considerable practical importance. It is especially significant since the subjects used in these experiments tended to be of above-average intelligence (Dale 1958 c). The increased demand for maintenance personnel inevitably means that men of lower intelligence than those hitherto accepted will have to be recruited and trained. Under present-day training methods it is known that they will have difficulty in assimilating electronic theory. These results indicate that even if they surmount this hurdle they cannot be expected to use such good strategies of searching as their more intelligent colleagues, at any rate, not without special training. (The effectiveness of the fault-finders of today might well be as high as it is because the men have been selected on the basis of intelligence in order to cope with electronics theory.)

The question arises whether, since men cannot be expected in general to use the best strategies, it might be better if the choice of checks was made for them. Warren *et al.* (1958) and Berkshire (1954) have developed guides on the assumption that this is so. It is really outside the scope of this paper to answer this question since much depends upon the conditions under which men work, but since learning occurs in the laboratory in a very short time there is good reason to hope that men can be trained to use reasonably efficient procedures.

§ 11. The Problem of Training

The results of the experiments reported here suggest that it is probably possible to train men to search reasonably efficiently. Some other studies have been specifically devoted to training, both in the laboratory and in the field.

In the laboratory, Fattu and Mech (1953) found that subjects who had had a lecture on a specific method of locating faults in a gear train apparatus successfully located more faults than others who had not had the lecture. Goldbeck *et al.* (1957) using an apparatus which presented flow problems of the kind used in the experiments reported above, found that subjects could be taught to dichotomize (at the same time they showed that the subjects could be confused by other aspects of the task). Warren *et al.* (1958) also report successful training using a simple simulated fault-finding task.

These experiments can be regarded as demonstrating the subjects' ability to understand and apply instructions. But more than this may be involved in establishing stable patterns of searching, for, if inefficient strategies are used because of a proclivity for risk-taking, then it will be necessary to thoroughly and effectively convince a man that, in the long run, gambling does not pay. Gambling has a strong and persistent appeal in everyday life, much of which might well lie in the pleasure of risk-taking itself rather than in the expectation of gain associated with the risks. Lord Keynes (1932), on these grounds, equated it with the pleasure of a glass of wine or a visit to the opera. Training men to abstain from risk-taking might, therefore, present a considerable problem. To instruct a man not to gamble is unlikely, by itself, to be effective. The trainee is likely to follow the instructions when tested, but this does not mean that he will continue to do so when he is no longer under observation. The car driving test provides a parallel. In the driving test the trainee must satisfy the examiner that, among other things, he drives cautiously. It is a matter of common observation, however, that this provides no guarantee that he will subsequently abstain from taking risks.

The evidence from field work, e.g. Anon 1953, and the complex laboratory tasks (Experiment I, Goldbeck *et al.* 1957, Fattu and Mech 1953) suggests that persons fail to understand how to set about searching. Instruction should help these persons. Evidence from the simple laboratory task, however, (Experiment II) suggests that inefficiency is due to the tendency to gamble. What might well be found, therefore, in the complex task, is that instructions, through giving an insight into the best ways of searching, will lead to an initial improvement; but when subjects master the task they will begin to take risks and inefficiency will result.

French *et al.* (1956) have been able to improve the fault-finding performance of electronics mechanics by specific training in which consideration of the basic signal flow characteristics of an equipment was emphasized. A follow-up was made after 6 months, to check that the improvement was maintained, but it was inconclusive; supervisor ratings did not distinguish between men with specific training and controls, but a written test did. More and better studies of this kind are badly needed.

In § 2 no definition of the task of fault-finding was offered since many approaches to the job are possible. The most efficient was described in order to show the scope of the job and this was then used as an ideal with which the strategies of the experimental subjects were compared. Lacking evidence to the contrary it seems reasonable to believe that men can be trained to approach this ideal method even if they do not match it. There might, however, be an intellectual barrier; it might be possible to train only men of relatively high intelligence.

What is striking about all the evidence discussed in this paper is the need for training which has been revealed by both on-the-job and laboratory studies. It is surprising that courses designed to produce fault-finders should, in fact, produce men who complain that they have not been taught how to set about locating faults. So much emphasis has been placed upon the theory which it has assumed the men will require, that the actual task has been forgotten. Indeed it has only recently been analysed at all. Proposals have

been made to correct this bias in training by Rulon and Schweiker (1956) and the writer (Dale 1958 e). Experimental training programmes based upon a job-orientated approach are briefly described by Ellis (1958). Unfortunately the research Ellis describes has been discontinued and the training he reports has not been properly evaluated, despite signs of considerable promise. It remains to be seen, therefore, how successful a training programme of this kind can be.

The virtues of the work reported here reflect the foresight of Dr. N. H. Mackworth, who instigated the project, and whose constant encouragement has greatly assisted its progress. Its deficiencies are due entirely to the writer. The Royal Navy and the Army kindly provided subjects ; the writer is particularly indebted to Colonel R. T. Barfield, R.E.M.E., for his help in making men and facilities available.

L'instruction nécessaire aux hommes afin de leur permettre de déceler des défauts rapidement et correctement est un problème qui n'a que récemment été reconnu. Jadis l'on a cru que l'acquisition d'une connaissance de théorie électronique était une préparation suffisante pour ce travail, mais l'on a observé qu'en général les hommes instruits de cette façon n'emploient pas la meilleure méthode de trouver les défauts; ils commettent frequemment de graves erreurs de stratégie. L'emploi de travaux de laboratoire où les difficultés secondaires associés au travail actuel dûs à un mauvais système de ' mécanisation humaine ' et un manque de livres techniques et de diagrammes sont éléminés, montre que la plupart des hommes ne cherchent pas efficacement. Leur conduite est influencée, jusqu'à un degré considérable, par des éléments sans importance ; elle change si le travail est présenté d'une manière différente ; et des traits non pertinents au déploiement d'un travail donné peuvent affecter leur procédé.

La justesse de la stratégie de l'homme semble correspondre positivement à son intelligence. Ceci semble suggérer que si le niveau intellectuel des hommes recrutés pour l'instruction du décelage de défauts est réduit, alors non seulement auront-ils plus de difficulté à assimiler la théorie électronique, mais, au moment de se mettre à la tâche propre, ils emploieront des stratégies moins efficaces dans le décèlement des défauts. Le problème créé par la nécessité de les instruire à employer de meilleures stratégies est discuté.

Des expériences faites par l'auteur sont décrites et rapportés aux travaux précédents.

Die Tatsache, dass die Schulung von Menschen für ein schnelles und richtiges Entdecken von Fehlern ein Problem darstellt, ist erst vor kurzem erkannt worden. Früher dachte man, dass das Studium der Elektronentheorie eine ausreichende Vorbereitung für diese Arbeit darstelle. Die Erfahrung hat aber gezeigt dass die Leute, die auf eine solcher Weise vorbereitet waren, die Fehler nicht nach der bestmöglichen Methode suchen und oft ernste strategische Fehler begehen. Die Benützung von Laboratoriumsversuchen, in welchen die sekundären Schweirigkeiten, die mit der wirklichen Aufgabe verbunden sind, wie die unzureichende Anwendung der ' menschlichen Technik ' (human engineering) und dem Mangel an geeigneten Unterlagen, Buchern und Diagramen, ausgeschaltet ist, zeigt dass die meisten Leute nicht in einer wirksamen Weise nach den Fehlern suchen. Ihr Benehmen ist zu einem beachtlichen Ausmasse von nebensächlichen Faktoren beeinflusst ; es ändert sich wenn die Aufgabe in einer anderen Weise gestellt wird und Nebeneinflüsse beeinträchtigen ihre Arbeitsmethoden.

Es scheint, dass die Tauglichkeit der Strategie eines Arbeiters in direktem Zusammenhang mit seiner Intelligenz steht. Das bedeutet, dass, falls weniger begabte Leute zum Finden von Fehlern angestellt werden, sie nicht nur grossere Schwierigkeiten in der Aneignung der Elektronentheorie haben, sondern auch später beim Fehlersuchen eine wirksame Strategie anwenden werden. Das Problem ihrer Schulung, mit dem Zeil ihnen bessere Methoden des Fehlersuchens zu lehren, wird behandelt.

Die Versuche des Verfassers uns ihr Zusammenhang mit seinen früheren Arbeiten wird beschrieben.

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ERGONOMICS RESEARCH SOCIETY

Some indication of the nature and purposes of the Society has been given in the editorial in the November issue of this Journal. Further particulars of the aims of the Society and of qualifications for membership are given by the Rules which are printed below.

Application for membership or for affiliation should be made in the first instance to the Hon. Secretary, Dr. O. G. Edholm, Division of Human Physiology, Medical Research Council Laboratories, Holly Hill, Hampstead, London, N.W.3.

GENERAL RULES OF THE SOCIETY

1. The Society shall be called " The Ergonomics Research Society ".

2. The object of the Society is to promote the study of the relation between man and the environment in which he works, particularly the application of anatomical, physiological and psychological knowledge to the problems arising therefrom.

Membership

3. The Society will consist chiefly of, and be governed exclusively by Ordinary Members, who alone will have power to elect members and officers, and to change the rules. The number of Ordinary Members shall not exceed one hundred and fifty or such larger number as the Council may from time to time decide, subject to confirmation by the Ordinary Members at the next Annual General Meeting.

4. Only those persons who work or have worked in the field of Ergonomics as defined in Rule 2, and agree in writing to become members, are eligible for election as Ordinary Members of the Society.

5. In addition to Ordinary Members, the Society may elect not more than ten Honorary Members. Persons of distinction who have contributed to the advancement of the aims of the Society are eligible for election as Honorary Members on the nomination of the Council.

6. Honorary Members shall have all the rights of Ordinary Members except that of voting.

7. Firms or Associations which have an interest in Ergonomics may become affiliated to the Society. Affiliated organizations may send two representatives to all Scientific Meetings of the Society, and they will receive one copy of all circulars, programmes and publications as sent to Ordinary Members.

Nomination and Election

8. Every nomination for Ordinary Membership of the Society shall give the full names and address of the candidate, also any degrees or diplomas which he/she may hold and the branch of work in which he/she is or has been engaged, together with a list of publications. Every candidate shall be recommended by not less than two members in the following terms :—

> A. B. having expressed a wish to join the Ergonomics Research Society we, the undersigned, from our personal knowledge, recommend him/her as a proper person to become one of its members.

9. Nominations made in the terms prescribed in Rule 8 shall be given in writing to the Honorary Secretary.

10. The Council shall, from the nominations submitted in accordance with Rules 8 and 9, prepare and cause to be submitted to any Meeting of the Society a list of approved candidates for election by the Ordinary Members of the Society at that meeting.

11. Nominations for Honorary Membership of the Society in accordance with Rule 5 shall be submitted to the Annual General Meeting of the Society.

12. At any meeting of the Society to elect members, if election by ballot be demanded by any member, one adverse vote in six shall exclude.

13. A person who has been elected a member of the Society shall be so informed by the Honorary Treasurer and Membership Secretary and shall be sent a copy of the Rules.

14. Every application for affiliation to the Society shall give the full name, address and nature of business of the organization. Applications will be considered by the Council which may accept or reject such applications.

15. The acceptance by the Council of an application for affiliation does not entitle the organization concerned to describe itself as a member of the Society nor to use the Society's name in any way as to suggest that the Society approves of any product of or action by the organization.

Subscriptions

16. The Annual subscription for Ordinary Members shall be Thirty Shillings, or such other rate as shall from time to time be determined by the Ordinary Members of the Society in General Meeting, in accordance with Rule 46.

17. The annual fee for affiliation shall be £5 5*s*. 0*d*.

18. The annual subscription and the annual affiliation fee are due on 1st January of each year and are payable in advance.

19. Every person elected as an Ordinary Member shall pay to the Honorary Treasurer and Membership Secretary the annual subscription for the current year. Members elected after 30th September in any year shall not be called upon to pay another subscription until January 1st of the second year following that in which they were elected.

20. Any member whose subscription has not been paid for one year and has been informed in writing of the fact by the Honorary Treasurer and Membership Secretary shall cease to be a member, unless in any particular instance, on the recommendation of the Council, the Society shall determine otherwise.

21. Resignation of membership shall be signified in writing to the Honorary Treasurer and Membership Secretary, but the member so resigning shall be liable for the payment of his annual subscription for the current year, together with any arrears up to the date of his resignation.

22. The provisions of Rules 19, 20 and 21 shall apply also to organizations affiliated to the Society.

Officers and Council

23. The officers of the Society shall consist of an Honorary Secretary and an Honorary Treasurer and Membership Secretary.

24. The business of the Society shall be carried on by a Council of members of the Society, consisting of the Officers and a Chairman of Council together with eight other Ordinary Members of the Society.

25. The Officers, Chairman of Council and Ordinary Members of the Council shall be elected or re-elected annually.

26. Two auditors shall be elected annually ; they shall not be members of the Council.

27. No Officer or Chairman of Council shall continue to hold office for more than seven consecutive years. Ordinary members of the Council shall not continue to serve for more than four consecutive years.

28. A quorum at a Council Meeting shall be five of whom one must be one of the Officers.

29. The Council shall have power to co-opt, until the next Annual General Meeting, not more than two other members to serve as additional members of the Council. (Rule 24 notwithstanding.)

30. The Funds of the Society shall be under the control of the Council who shall have the power to expend such funds for the promotion of the objects of the Society as they think fit.

31. The Council may form Sub-Committees consisting of such members of the Society as it thinks fit and may delegate any of its powers to such Sub-Committees.

32. It shall be the duty of the Council to propose to the Society for election or re-election names of members to fill the offices laid down in Rules 23 and 24. The names so proposed shall be submitted to the Ordinary Members of the Society six weeks before the Annual General Meeting. Other nominations for the Council made by Ordinary Members shall be seconded by two Ordinary Members and forwarded in writing to the Honorary Secretary at least one month before the Annual General Meeting, together with the consent of the nominee. If no fresh nominations are received, the Council's nominees will thereby be deemed to be elected. Otherwise, election shall be by Ballot at the meeting.

33. Any vacancy occurring in the Council between Annual General Meetings may be filled by an Ordinary Member elected by the Council. The member so elected shall retire at the end of the year but shall be eligible for re-election by the Society at the next Annual General Meeting, Rule 27 notwithstanding.

The Honorary Secretary

34. The duties of the Honorary Secretary shall be to arrange the business and scientific meetings of the Society in accordance with the directions of the Council, and to notify members of the time and place of meetings, to attend such meetings and also meetings of the Council and take minutes and to read these minutes and likewise any letters or reports.

35. The Honorary Secretary shall prepare an annual report upon the activities of the Society for submission to the Annual General Meeting.

The Honorary Treasurer and Membership Secretary

36. The Honorary Treasurer and Membership Secretary shall keep an up-to-date list of members of the Society and publish this list from time to time to the Members of the Society in accordance with the direction of the Council. The list shall indicate as Founder Members those members who were present at the meeting convened at Oxford on 30th September 1949, or who, being invited to attend, were unable to do so but expressed in writing their desire to become members of the Society.

37. The Honorary Treasurer and Membership Secretary shall have charge of the funds of the Society, receive sums due to it and pay such bills as are directed by the Council to be discharged.

38. The Honorary Treasurer and Membership Secretary shall make up the accounts of the Society to the 31st December in each year, and present at the Annual General Meeting an Income and Expenditure Account and a Balance Sheet duly certified by the Auditors.

The Conduct of Meetings

39. Unless the Council decide otherwise, there shall be not less than three meetings in each year. One of these meetings shall be held in the first six months of the year and shall be the Annual General Meeting.

40. The ordinary meetings of the Society shall be scientific meetings. In addition at the Annual General Meeting, the administrative business of the Society shall be conducted. The Chairman of the Annual General Meeting or Special General Meeting shall be a member of the Council.

41. The Council shall make arrangements for the presentation and discussion of communications and demonstrations and all other matters relating to the expeditious conduct of scientific meetings.

42. At Annual or Special General Meetings a quorum shall be twenty persons or by proxy of which at least twelve shall be present in person.

43. Any Ordinary Member unable to attend an Annual or Special General Meeting may vote by proxy or by post by sending a completed and signed voting paper to one of the Secretaries before the day of the meeting. The Chairman of the meeting shall add such proxy and postal votes to those recorded by members attending in person.

44. The notice convening meetings shall give not less than twenty-one days' notice of the date of the meetings and the notice convening the Annual General Meeting shall give six weeks' notice. The Agenda for the Annual General Meeting shall be sent out three weeks before the date of the Meeting.

45. The Honorary Secretary may call a Special General Meeting at the request of the Council or of ten members of the Society. Such a meeting shall be called so as to give at least one month's notice of the date of the meeting and the purpose for which it is called shall be explicitly stated in the notice convening the meeting ; no other business shall be transacted thereat.

46. The rules of the Society shall not be changed unless three-quarters of those voting are in favour of such change at an Annual or Special General Meeting. Notice of the suggested change must be given to the Honorary Secretary at least one month before such Annual or Special General Meeting, and he shall notify all Ordinary Members of the suggested change at least three weeks before the meeting.

47. Not more than one visitor may be introduced by each member of the Society at any ordinary meeting, provided due notice has been given to the Honorary Secretary beforehand. The Council shall have power to suspend or modify this rule.

Publication

48. A programme of each ordinary meeting shall be sent to all members at least seven days before the meeting. The programme shall include as far as possible the title of each Demonstration or Communication and, at the option of the author, may be accompanied by a brief account not exceeding 250 words. Copies of the programmes and abstracts of communications shall be preserved by the Honorary Secretary. These accounts shall not be regarded as publications and may not be quoted without the author's consent.

49. No report of the proceedings at any meeting of the Society shall be taken or published unless the consent of the Council has been previously obtained.

INDEX OF AUTHORS

WITH THE TITLES OF PAPERS

SUBJECT INDEX

Keywords have been selected from the titles of papers. The full titles are given in the Index of Authors.

INSTRUCTIONS TO CONTRIBUTORS

1. Articles for publication should be sent to the General Editor or to any Member of the Editorial Board.

2. Papers must be in English, French or German. Every paper must be accompanied by a brief summary, and contributors are asked if possible to supply summaries in all three languages.

3. Articles must be concise and should not normally exceed 6000 words.

4. Authors should submit a typescript, double-spaced on one side of the paper only. Footnotes should be avoided. Summaries, tables and legends for diagrams should be typed on separate sheets. Authors must ensure that the lay-out of mathematical and other formulae is clear. The typescript must represent the final form in which the author wishes the article to appear. The cost of any alteration in proof other than printers' errors may be charged to the author.

5. Diagrams should be drawn in black ink on white card or tracing paper. They should normally be sufficiently large to allow reduction in printing and the lines should therefore be bold. **All lettering should be up to draughtsmanship standard, suitably drawn in Indian ink to allow for reduction in size.** No charges are made for reproducing tables, diagrams or half-tone illustrations, but diagrams not suitable for reproduction without redrawing may be redrawn at the Author's expense.

6. References in the text should be indicated by author's name followed by the date. They should be listed alphabetically at the end of the paper in the style illustrated by the following examples:

BARTLETT, F. C., 1943, Fatigue following highly skilled work. *Proc. roy. Soc.* B, **131,** 247–254.

BEDFORD, T., 1948, *Basic Principles of Ventilation and Heating* (London: H. K. Lewis).

LE GROS CLARK, W. E., 1954, The anatomy of work. In *Symposium on Human Factors in Equipment Design* (Edited by W. F. Floyd and A. T. Welford) (London: H. K. Lewis). Pp. 5–15.

Abbreviations should be as in the *World List of Scientific Periodicals.*

7. **Consideration for publication will gladly be given to papers which have previously had a limited circulation as research reports.** Submission of a paper implies, however, that it has not been published and will not be published elsewhere without the permission of the General Editor and the Publishers. Copyright in material accepted for publication is retained by the Journal, and reproduction in whole or in part is forbidden except under the terms of the *Fair Copying Declaration* of the Royal Society or with the written permission of the Publishers.

8. Authors will receive 25 copies of their contributions without charge. Additional copies may be ordered at the time of returning proofs. Prices for additional copies may be obtained from the Publishers.

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